THE LONDON SCHOOL
OF ECONOMICS AND
POLITICAL SCIENCE ■

LSE Research **Online**

Xavier Jaravel, Neviana Petkova and Alex Bell
Team-specific capital and innovation

Article (Accepted version)
(Refereed)

This version available at: http://eprints.lse.ac.uk/87653/
Available in LSE Research Online: May 2018

http://eprints.lse.ac.uk

Team-Specific Capital and Innovation[*]

Xavier Jaravel, London School of Economics
Neviana Petkova, Office of Tax Analysis, US Department of the Treasury
Alex Bell, Harvard University

October 2017

Abstract

We establish the importance of team-specific capital in the typical inventor's career. Using administrative tax and patent data for the population of US patent inventors from 1996 to 2012, we find that an inventor's premature death causes a large and long-lasting decline in their co-inventor's earnings and citation-weighted patents (-4% and -15% after 8 years, respectively). After ruling out firm disruption, network effects and top-down spillovers as main channels, we show that the effect is driven by close-knit teams and that team-specific capital largely results from an "experience" component increasing collaboration value over time.

JEL codes: O31, O32, J24, J30, J41

Teamwork has become an essential feature of modern economies and knowledge production (Wuchty, Jones and Uzzi (2007); Jones (2010); Crescenzi, Nathan and Rodríguez-Pose (2016); Jaffe and Jones (2015); Seaborn.T (1979)). We investigate empirically the importance of *team-specific capital* for the compensation and patent production of inventors, using administrative tax and patent data for the population of US patent inventors from 1996 to 2012. Conceptually, while general human capital augments productivity at all firms (Becker (1975)), and while firm-specific capital augments productivity with any existing or future collaborators within the firm(Topel (1991)), the idea of team-specific capital is that an inventor may be more productive with their existing co-inventors. Team-specific capital encompasses skills, experiences and knowledge that are useful only in the context of a specific collaborative relationship: high team-specific capital means that the collaborative dynamics in the team are unique and difficult to rebuild with other collaborators, which improves each inventor's ability to produce valuable innovations with these specific co-inventors. If the collaboration between two patent inventors were to exogenously end, would this have a significant and long-lasting impact on the career, compensation, and patents of these inventors? Or are co-inventors easily substituted for, beyond short-term disruption of ongoing work? In other words, is team-specific capital an important ingredient of the typical inventor's lifecycle earnings and patents, much like firm-specific capital is crucial for the typical worker? This paper establishes the existence, nature and economic relevance of patent inventors' team-specific capital.

We provide causal estimates of what the typical inventor would lose, in terms of labor earnings, total earnings and patent production, if a collaboration with one of their co-inventors were to end exogenously. Using a detailed merged dataset of United States Patent and Trademark Office (USPTO) patents data

and Treasury administrative tax data, we use the premature deaths of 4,714 inventors, defined as deaths that occur before or at the age of 60, as a source of exogenous variation in collaborative networks. The causal effect is identified in a difference-in-differences research design, using a control group of patent inventors whose co-inventors did not pass away but who are otherwise similar to the inventors who experienced the premature death of a co-inventor. We find that ending a collaboration causes a large and long-lasting decline in an inventor's labor earnings (- 3.8% after 8 years), total earnings (- 4% after 8 years) and citation-weighted patents (- 15% after 8 years). This evidence implies that the continuation of collaborative relationships has substantial specific value for the typical inventor, approximately equal to half of the returns to one year of schooling (Mincer et al. (1974)). It rejects the alternative hypothesis that continued collaborations are not a key ingredient in an inventor's earnings function and patent production function beyond short-term disruption of ongoing work.

To establish team-specific capital as the primary explanatory mechanism, we show that the decline in earnings and citation-weighted patents following the premature death of a co-inventor is driven by the fact that the inventor lost a partner with whom they were collaborating extensively, which made additional co-inventions impossible. We do so in four steps. First, we rule out alternative explanatory mechanisms that are not specific to the team. In particular, we establish that the effect does not stem from the disruption of the firm or from network effects by estimating the causal effect of an inventor's death on their coworkers and on inventors that are two nodes away from the deceased in the co-inventor network.[1] Second, we show that although

---

[1] In our data, firms are proxied for by tax Employer Identification Numbers (see Section II for a complete discussion). In addition to ruling out important alternative mechanisms that

top-down spillovers from unusually high-achieving deceased inventors are important (consistent with Azoulay, Graff Zivin and Wang (2010); Oettl (2012)), they are not driving the average effect we document. Third, we demonstrate that the intensity of the collaboration between an inventor and their deceased co-inventor prior to death is an important predictor of the magnitude of the effect. Fourth, we document that the effect of co-inventor death on an inventor's patents is much smaller when patents that were co-invented with the deceased are not taken into account in the difference-in-differences analysis: although the survivor's own patents suffer as well, the effect primarily applies to co-invention activities with the deceased.[2]

Finally, we investigate how team-specific capital is formed and how it increases inventors' earnings and patents. We use heterogeneity in the treatment effect to test the implications of various possible models of team-specific capital. We reject a broad class of search-and-matching models in which team-specific capital is conceptualized as resulting from a "match" component which is constant over time, for instance when two inventors are a particularly good fit for each other. In contrast, we find support for the view that team-specific capital accumulates during a collaboration and results from an "experience" component which increases the value of the collaboration over time, for example when two inventors learn how to best collaborate with each other over the course of several joint projects.

Our work relates to several strands of literature. The use of premature

---

could explain our finding, the analysis of firm and network effects yields new insights about substitution and complementarity patterns between inventors in the innovation production function (see Section IV for a complete discussion).

[2]We also show that team-specific capital matters in all technology categories, at various levels of the distribution of patent quality, and spans the boundaries of commuting zones and firms. In Section IV, we discuss whether other mechanisms could be consistent with the evidence.

deaths as a source of identification is becoming increasingly common (Jones and Olken (2005); Bennedsen et al. (2007); Azoulay, Graff Zivin and Wang (2010); Nguyen and Nielsen (2010); Oettl (2012); Becker and Hvide (2016); Fadlon and Nielsen (2015); Isen (2013)) and several papers have investigated peer effects in specific areas of science: Agrawal, Kapur and McHale (2008); Borjas and Doran (2012, 2015); Oettl (2012); Waldinger (2010, 2011). Our paper is the first to study collaboration effects by looking at both earnings and innovation outcomes. Our results are consistent with the findings that direct collaborators matter, as in Azoulay, Graff Zivin and Wang (2010) and Borjas and Doran (2015) , but also that there are no wider firm-specific or university-specific spillovers, as in Waldinger (2011). We estimate the differential spillover effect of an inventor on various peer groups (co-inventors, coworkers, and second-degree connections in the co-inventor networks) using the same research design, which allows us to establish the unique importance of co-inventors in an inventor's career. Other related strands of literature study the role of teams in innovation (e.g. De Dreu (2006); Jones (2009); Agrawal, Kapur and McHale (2008); Alexander and Van Knippenberg (2014)), examine the notion of team-specific or network-specific human capital from a theoretical perspective (e.g. Mailath and Postlewaite (1990); Chillemi and Gui (1997)) , investigate the effect of co-mobility of colleagues (Hayes, Oyer and Schaefer (2005); Groysberg and Lee (2009); Campbell, Saxton and Banerjee (2014)) and develop theories of knowledge spillovers across inventors (e.g. Stein (2008); Lucas and Moll (2014)). Finally, this paper is part of a nascent literature using administrative data to describe the careers of patent inventors (Toivanen and Väänänen (2012); Bell et al. (2016); Dorner et al. (2014); Depalo, Addario and Lucia (2014); Aghion and Howitt (1992)).

The remainder of the paper is organized as follows. In Section II, we present

the dataset and novel descriptive statistics on the composition of teams. In Section III, we describe the research design and present the estimates of the causal effect of the premature death of a co-inventor on an inventor's compensation and patents. In Section IV, we establish that team-specific capital is a central explanatory channel, ruling out alternative mechanisms that do not operate within teams. In Section V, we present a series of results delivering insights about the workings of team specific capital. Section VI concludes. Several robustness checks, heterogeneity results and empirical estimation details are deferred to the Online Appendix.[3]

I. Data and Descriptive Statistics

A. Data Construction

We use a merged dataset of United States Patent and Trademark Office (USPTO) patents data and Treasury administrative tax files as inBell et al. (2016). The patent data are extracted from the weekly text and XML files of patent grant recordations hosted by Google. The raw files contain the full text of about 5 million patents granted from 1976 to today, extracted from the USPTO internal databases in weekly increments.

Administrative data on the universe of U.S. taxpayers is sourced from Treasury administrative tax files. We extract information on inventors' city and state of residence, wages, employer ID, adjusted gross income, as well as current citizenship status and gender from Social Security records. Most data are

---

[3]Appendix A reports additional summary statistics and tests for balance between treated and control groups. Appendix B presents robustness checks on the causal effect of co-inventor death. Appendix C conducts additional tests for heterogeneity in the effect of co-inventor death. Appendix D provides additional results on the nature of team-specific capital. Appendix E provides more details on our econometric framework. Appendix F describes the construction of the dataset and reports additional summary statistics on the composition of inventor teams.

available starting in 1996, however wages and employer ID are available only starting in 1999, which marks the beginning of W-2 reporting. Inventors from the USPTO patent data are matched to individual taxpayers using information on name, city and state of residence (Appendix A describes the iterative stages of the match algorithm). The match rate is over 85% and the matched and unmatched inventors appear similar on observables, as documented inBell et al. (2016). Any inventor with a non-U.S. address in the USPTO patent data is excluded from the matching process and dropped from the sample. The resulting dataset is a panel of the universe of U.S.-based inventors, tracking over 750,000 inventors from 1996 to 2012, which we refer to as the "full sample" of inventors for the remainder of the paper.

The employer ID is based on the Employer Identification Number (EIN) reported on W-2 forms. In some cases, it could be that business entities with different EINs are the subsidiary of the same parent company, therefore business entities with distinct EINs are not necessarily distinct firms.

B. Identifying Deceased Inventors, Survivor Co-inventors, Second-Degree Connections and Coworkers

We construct various groups of inventors to carry out the premature death research design. We start by identifying 4,924 inventors who passed away before or at the age of 60 and were granted a patent by USPTO before their death.[4] Information on the year of death and age at death is known from Social Security records. The cause of death is not known. In order to reduce the likelihood that death results from a lingering health condition, we consider

---

[4]As described below, ultimately we analyze only 4,714 premature deaths due to the lack of appropriate matches for the remaining prematurely deceased inventors. We consider prematurely deceased inventors who are weakly below 60, i.e. we keep inventors who are 60 in the year of death.

inventors passing away before or at the age of 60 and, in robustness checks, we repeat the analysis by excluding deceased inventors who ever claimed tax deductions for high medical expenses.

We construct a group of "placebo deceased inventors" who appear similar to the prematurely deceased inventors but did not pass away. Specifically, we use a one-to-one exact matching procedure on year of birth, cumulative number of patent applications at the time of (real or placebo) death, and year of (real or placebo) death in order to identify placebo deceased inventors among the full population of inventors.[5] 4,714 deceased inventors find an exact match using this procedure.[6] Thus, we obtain a control group of placebo deceased inventors who have exactly the same age, the same number of cumulative patent applications and exactly the same year of (placebo) death as their associated (real) deceased inventor.

Next, we build the co-inventor networks of the real and placebo deceased inventors. Any inventor who ever appeared on a patent with a real or placebo deceased inventor before the time of (real or placebo) death is included in these networks. In the rest of the paper, we refer to these inventors as real and placebo "survivor inventors." We exclude survivor inventors who are linked

---

[5]The match is conducted year by year. For instance, for inventors who passed away in 2000, we look for exact matches in the full sample of inventors. An exact match is found if the control inventor was born in the same year and had the same number of cumulative patent applications as the deceased in 2000. The inventors from the full sample that match are then taken out of the sample of potential matches, and the procedure is repeated for the following year, until the end of the sample. This matching procedure without replacement thus determines a counterfactual timing of death for the placebo deceased inventors. When there is more than one exact match, the ties are broken at random.

[6]The 5% unmatched deceased inventors do not significantly differ on observable characteristics from those who find a match, except that they tend to have more cumulative applications at the time of death. In robustness checks presented in Appendix E, we repeat the analysis with a propensity-score reweighting approach which uses data on all deceased inventors and obtain similar results.

to more than one real or placebo deceased inventor.[7] We thus obtain 14,150 real survivor inventors and 13,350 placebo survivor inventors. These inventors constitute the main sample used for the analysis carried out in the rest of the paper. Note that we perform the matching procedure on the real and placebo deceased inventors rather than on the survivor inventors - the benefits of this approach are discussed in Section III.

We construct two other groups of inventors, which will be used to differentiate between mechanisms. First, we build the network of inventors who are two nodes away from the real and placebo deceased inventors in the co-inventor network. These inventors are direct co-inventors of the deceased's direct co-inventors, but they never co-invented a patent with any of the (real or placebo) deceased inventors. To increase the likelihood that these inventors were never directly in contact with the deceased, we impose two additional restrictions: of the inventors who are two nodes away from the deceased in the co-inventor network, we keep only those who never worked for the same employer and never lived in the same commuting zone as the deceased inventor. We refer to these inventors as real and placebo "second-degree connections" for the remainder of the paper. As before, we exclude inventors in this group who are linked to more than one real or placebo deceased inventors. This procedure yields 11,264 real second-degree connections and 12,047 placebo second-degree connections. Second, we construct the group of "coworkers" of the deceased by identifying all inventors who worked for the same employer as the deceased in the year before death, as indicated on W-2 forms. We exclude coworkers that ever co-invented with a prematurely deceased inventor or who experienced multiple premature death events. Focusing on coworkers in firms with less then 2,000 employees, the final sample consists of 13,828 real coworkers

---

[7]We lose only 36 survivor inventors by imposing this restriction.

and 14,364 placebo coworkers.[8]

C. Variable Definitions and Summary Statistics on Inventors

In the analysis carried out in the rest of the paper, we study various outcome variables at the individual level from 1999 until 2012. First, we consider inventors' labor earnings, which refer to annual W-2 earnings. When an inventor does not receive a W-2 form after 1999, we impute their labor earnings in that year to be zero. Second, we construct a measure of an inventors' total earnings, defined as an inventors' adjusted gross income (earnings reported on IRS tax form 1040) minus the W-2 earnings of the inventor's spouse. Adjusted gross income is a tax concept offering a comprehensive measure of a household's income, including royalties, self-employment income and any other source of income reported on 1040 tax forms.[9] We define non-labor earnings as the difference between total earnings and labor earnings. All earnings variables are winsorized at the 1% level.[10] Third, we use adjusted forward citations, which are defined for year t as the total number of forward citations received on all patents the individual applied for in year t, divided by the number of inventors who appear on each patent. Forward citations include all citations

---

[8]We focus on smaller firms to increase the chances that we find a negative effect of an inventor's death on their coworkers, since we are interested in testing whether the effect we document for co-inventors is driven by the disruption of the firm. In Appendix C, we carry out the analysis on the full sample of coworkers, composed of 173,128 real survivor coworkers and 143,646 placebo survivor coworkers, and we find similar results. The difference in the size of the groups of real and placebo coworkers in the full sample is driven by a thin tail of deceased inventors working in firms employing thousands of other inventors, as documented in Appendix Table A5.

[9]A limitation of our measure of total earnings for inventors filing jointly is that we can only subtract the inventor's spouse's W-2 earnings from the household's adjusted gross income, not the spouse's other sources of income, which are unobserved. But the exact same procedure is applied to all inventors in the various groups we consider. Another limitation is that adjusted gross income does not include tax-exempt interest income.

[10]We have checked that the results are robust to winsorizing at the 5% level and that we obtain similar results when we do not winsorize (see Appendix Table B14).

of the patent made as of December 2012 and are a measure of the "quality" of innovative output. We divide forward citations by the total number of inventors on the patent to reflect the fact that a single inventor's contribution is smaller in larger teams.[11] Fourth, we use the number of patents granted by the USPTO as of December 2012, as well as the number of patents in the top 5% of the citation distribution.[12] Lastly, we create indicator variables that turn to one when labor earnings are greater than 0 or above thresholds for the 25th, 50th and 75th percentiles of the labor earnings distribution.[13] We proceed similarly for total earnings. These indicators are used as outcome variables to characterize the effect of an inventor's premature death on their co-inventors' compensation at different quantiles of the income distribution. Since labor earnings are only available from 1999 onwards, for consistency we do not use data prior to 1999 for any of the variables in the analysis, but the results are qualitatively similar when pre-1999 data are included for adjusted gross income, patent applications and citations.

Panel A of Table 1 presents summary statistics for the variables of interest in the main samples used in the analysis. Statistics on total earnings and wages are computed based on the entire panel for the full sample of inventors,

---

[11]This is common practice. We check the robustness of our results with other measures of citations, which do not adjust for team size, take into account citations only over a fixed rolling window of a couple years around application or grant (in order to address truncation issues), and distinguish between examiner-added and applicant-added citations. Section III discusses these various robustness checks.

[12]We define the count of patents in the top 5% of citations as the number of patents the survivor inventor applied for in a given year that were in the top 5% of the citation distribution, where the distribution is computed based on all patents that were cited, applied for in the same year and in the same technology class (we aggregate USPC classes into six main technology classes, as is common in the literature). Throughout the paper, we consider only patents that were granted as of December 2012 and we use the year of filing of the patent application as the year of production of the invention.

[13]These quantiles are computed before the time of death in the population of real and placebo survivor inventors.

and based on years before the death event for the deceased and the survivor inventors. Age, cumulative applications and cumulative citations are computed in the year of death for the deceased and the survivors, and across all years for the full sample. Panel B of Table 1 presents similar statistics for the second-degree connections and coworkers. Appendix Tables A1 and A2 report more detailed summary statistics, showing the full distribution of the various outcomes for each group of inventors.

The real deceased inventors are on average seven years older than inventors in the full sample. By construction, the mean and distribution of age at death for the placebo deceased inventors exactly match that of the real deceased inventors. Likewise, the mean and distribution of the number of applications is the same for real and placebo deceased inventors. The means and distributions of labor earnings, total earnings and forward citations are also very similar in these two groups, although our matching algorithm did not match on these variables. The real and placebo survivor inventors are also older than inventors in the full sample and they have much higher labor earnings and total earnings and many more patent applications and citations. The age difference is due to the fact that there is assortative matching by age in inventor teams, as discussed in Section II.D, and the deceased are older than inventors in the full sample. The difference in compensation and patents is due to a selection effect: inventors who have co-invented many patents are more likely to experience the (real or placebo) death of one of their co-inventors. Therefore, it would not be appropriate to use the full population of inventors as a control group for the real survivor inventors, as their lifecycle earnings are likely to be on different trajectories. In contrast, the means and distributions of labor earnings, total earnings, age and patent applications and citations are very similar in the gr-

11

Table 1—Summary Statistics on Inventors

Panel A. For Main Analysis

Variable	Sample	Mean	SD
Total Earnings	Full Sample	144,096	316,636
	Real Deceased	139,857	308,000
	Placebo Deceased	139,102	320,970
	Real Survivors	177,020	355,347
	Placebo Survivors	177,247	360,780
Labor Earnings	Full Sample	117,559	257,466
	Real Deceased	121,691	258,289
	Placebo Deceased	124,149	248,546
	Real Survivors	152,602	295,832
	Placebo Survivors	155,098	290,201
Cumulative Applications	Full Sample	2.31	2.51
	Real Deceased	2.50	2.43
	Placebo Deceased	2.50	2.43
	Real Survivors	12.42	28.31
	Placebo Survivors	11.92	29.52
Cumulative Citations	Full Sample	6.64	12.2
	Real Deceased	8.74	13.09
	Placebo Deceased	8.51	13.20
	Real Survivors	42.00	171.03
	Placebo Survivors	40.20	164.20
Age	Full Sample	43.29	9.65
	Real Deceased	50.85	7.44
	Placebo Deceased	50.85	7.44
	Real Survivors	47.53	10.89
	Placebo Survivors	47.289	11.16
# Inventors	Full Sample	756,118	
	Real Deceased	4,714	
	Placebo Deceased	4,714	
	Real Survivors	14,150	
	Placebo Survivors	13,350	

Table 1—Summary Statistics on Inventors*(continued)*

Panel B. For Additional Analysis

Variable	Sample	Mean	SD
Total Earnings	Real 2nd-degree Connections	175,247	358,347
	Placebo 2nd-degree Connections	174,900	350,102
	Real Coworkers	149,861	312,721
	Placebo Coworkers	154,627	316,266
Labor Earnings	Real 2nd-degree Connections	144,449	291,697
	Placebo 2nd-degree Connections	146,674	297,697
	Real Coworkers	114,559	258,233
	Placebo Coworkers	117,691	256,908
Cumulative Applications	Real 2nd-degree Connections	10.42	42.78
	Placebo 2nd-degree Connections	9.92	25.21
	Real Coworkers	2.40	2.58
	Placebo Coworkers	2.45	2.52
Cumulative Citations	Real 2nd-degree Connections	37.76	170.11
	Placebo 2nd-degree Connections	39.40	173.23
	Real Coworkers	5.74	11.62
	Placebo Coworkers	6.05	12.19
	Real 2nd-degree Connections	47.72	19.08
	Placebo 2nd-degree Connections	47.93	19.96
	Real Coworkers	44.28	12.94
	Placebo Coworkers	44.49	16.13
# Inventors	Real 2nd-degree Connections	11,264	
	Placebo 2nd-degree Connections	12,047	
	Real Coworkers	13,828	
	Placebo Coworkers	14,364	

Notes: This table reports summary statistics for the various groups of inventors defined in Section II.B. The statistics for the full sample are computed using data from 1999 to 2012. For the deceased and survivor inventors, as well as the second-degree connections and co-workers, the statistics are computed using data before the year of death. Dollar amounts are reported in 2012 dollars. Appendix Tables A1 and A2 report more detailed summary statistics, showing the full distribution of outcomes. For a detailed description of the data sources and sample construction, see Sections II.A and II.B.

-oup of placebo survivors and real survivors. Importantly, our matching algo-

rithm did not impose that any of the characteristics of the placebo survivor inventors should be aligned with those of the real survivor inventors, since we matched on characteristics of the real and placebo deceased only. The Labor earnings are slightly lower for the real survivors compared to the placebo survivors, but we will check in Section III that this difference is constant during years prior to co-inventor death, consistent with the assumptions of the difference-in-differences research design. Appendix Tables A3 and A4 show that the real and placebo survivors are also similar in terms of the year of co-inventor death, their technology class specialization, the size of their co-inventor networks and the size of their firms.

Finally, Panel B of Table 1 shows that the populations of real and placebo second-degree connections are similar to the survivor inventors, while the outcomes for real and placebo coworkers are close to those of the full sample.

D. Descriptive Statistics on Patent Inventor Teams

Teams of inventors keep growing in importance. The number of inventors listed on a patent has been growing over time and in our sample patents with a single inventor account for about 35% of all patents — all other patents are produced by teams, with teams of relatively small sizes (e.g. two or three inventors) accounting for the largest share of patents. Panels A and B of Appendix Figure A2 present these facts and Appendix Table A8 indicates that the patterns are similar across technology classes.

As shown on Panel B of Appendix Figure A2, the distributions of team sizes for real and placebo survivors track each other very closely, although our matching algorithm did not use any information on team composition. These distribution clearly differ from that of the full sample, which is due to a selection effect: inventors who tend to work more in teams, and especially in

larger teams, have more co-inventors and hence are more likely to experience the premature death of one of them.

Teamwork is common, but inventors are more rarely part of multiple teams. To establish this, we build team identifiers, where a team is defined as a unique combination of (two or more) inventors listed on a patent. Panel A of Table 2 shows that the median number of teams per inventor is just one, although there is a thick tail of inventors belonging to many teams. This panel also shows that there is a high degree of overlap across teams. Considering inventors who are part of at least two teams, on average the percentage of overlapping co-inventors between two teams that any given inventor belongs to is 45%. This number is a bit lower for real and placebo survivors relative to the full sample, again due to a selection effect: it is more likely for an inventor to experience co-inventor death if they have more distinct co-inventors.

Panel B of Table 2 shows that the composition of teams is very heterogeneous. First, teams members are not always co-located in the same commuting zone. The degree of geographic dispersion increases with team size, although for all team sizes at least 25% of co-inventors reside in the same commuting zone.[14] Second, team members can be very heterogeneous, in a way that is not well predicted by team size. Panel B shows this by reporting the distribution of the coefficient of variation for total earnings *within* teams, for various team sizes. Within-team heterogeneity increases with team size, but relatively little, while it greatly varies holding team size constant. Similar results hold with other proxies for within-team heterogeneity, using other dispersion metrics (standard deviation and Herfindahl index) and other outcomes (labor earnings, applications, citations, age), as well as for the full sample of inventors, as

---

[14]Appendix Table A9 shows similar results for the full sample of inventors and at the state level.

15

reported in Appendix Tables A10 A11 and A12. For teams of two inventors, we study the extent of assortative matching non-parametrically using the absolute difference between outcomes for each of the co-inventors. The results, reported in Appendix Figure A3 and Appendix Table A13, show that inventors who are similar in characteristics like age and compensation tend to work together, but only up to a point: there is wide variation in the composition of inventor teams. Given the wide variety of team structures revealed by these summary statistics, in Section V we investigate the question of which team structures are most conducive to the accumulation of team-specific capital.

Appendix Table A6 presents descriptive evidence on team formation dynamics, from the point of view of the placebo survivors, around the time of (counterfactual) co-inventor death. The placebo survivors do not add many new co-inventors after the time of co-inventor death. Moreover, these new co-inventors account for only 25% of their total patents after co-inventor death, suggesting that the quality of these new matches is relatively low. These patterns are not very different across age groups, although it appears that younger inventors tend to add more co-inventors and innovate relatively more with them, as if team-specific capital were easier to accumulate earlier in an inventor's career.[15] Appendix Table A6 provides another illustration of the "stickiness" of teams, which was already evident in Panel A of Table 2: inventors work in a few teams only and tend to collaborate with the same co-inventors across teams. We will use these facts to motivate our analysis of possible mechanisms in Section V.

To further document that team composition features a significant degree of stickiness, we consider teams that applied for a patent in 2002, in the full

---

[15]Appendix Table A7 presents complementary evidence on the likelihood of switching EINs over time, from the perspective of the placebo inventors.

Table 2—Summary Statistics on Inventor Teams

Panel A. Inventor-Level Statistics on Collaborations

Variable	Sample	Mean	SD	10pc	25pc	50pc	75pc	90pc
Number of Teams	Full Sample	2.58	4.17	1	1	1	3	5
per Inventor	Real Survivors	2.83	4.45	1	1	1	3	6
	Placebo Survivors	2.79	4.09	1	1	1	3	6
Distinct Co-Inventors	Full Sample	2.32	3.0	1	1	1	3	5
per Inventor	Real Survivors	3.45	3.79	1	1	2	5	9
	Placebo Survivors	3.43	3.73	1	1	2	5	9
Degree of Overlap in Co-Inventors	Full Sample	45.32	29.15	16.66	25	33.33	50	100
across Teams, for Inventors in at	Real Survivors	31.82	20.84	12.90	18.33	25	37.5	50
Least Two Teams (%)	Placebo Survivors	32.13	21.10	13.22	18.75	26.08	37.5	50

Panel B. Team-Level Statistics for Real and Placebo Survivors, by Team Size

	Team Size	Mean	p10	p25	p50	p75	p90
	2	1.212	1	1	1	2	2
Number of Distinct	3	1.766	1	1	2	2	3
Commuting Zones	4	1.795	1	1	2	2	3
across Co-Inventors	5	2.057	1	1	2	3	3
	6	2.312	1	1	2	3	4
	2	0.391	0.051	0.150	0.330	0.609	0.945
Team Heterogeneity	3	0.434	0.065	0.158	0.346	0.612	0.999
(Coefficient of Variation for	4	0.414	0.084	0.199	0.372	0.611	0.950
Total Earnings, Within Team)	5	0.431	0.097	0.220	0.401	0.611	0.949
	6	0.439	0.107	0.229	0.415	0.640	1.012

Notes: Panel A reports summary statistics at the inventor level for the various groups of inventors defined in Section II.B. The statistics for the full sample are computed using data from 1999 to 2012. For the deceased and survivor inventors, the statistics are computed using data before the year of death. Panel B reports summary statistics at the team level, where a team is defined as a unique combination of more than two inventors listed on a patent. For each team, the outcomes are measured in the year of a random patent application prior to the year of death. See Appendix Table A9 for additional evidence on geographic dispersion and Appendix Tables A10,A11,A12,A13 and Appendix Figure A3 for additional evidence on within-team heterogeneity. For a detailed description of the data sources and sample construction, see Sections II.A and II.B.

sample of inventors, and find that the probability that another patent applied for by a member of the team between 1997 and 2007 also includes at least one

other member of the 2002 team is 30.4%. When conditioning on patents that were assigned to different assignees[16], the percentage falls but remains high, at 21.6%. This suggests that teams are persistent across firm boundaries.[17]

Overall, the summary statistics on teams confirm the similarity between real and placebo survivors and point to several directions for heterogeneity in treatment effect by team structure, which we investigate and relate to common hypotheses in the literature in Section V. Given that teams of two inventors are the most frequent, and given that co-inventors often move together across teams, we primarily conduct our causal analysis at the co-inventor level for the remainder of the paper.

II. Estimating the Causal Effect of the Premature Death of a Co-Inventor on an Inventor's Compensation and Patents

This section presents our methodology to estimate the average treatment effect of experiencing death of a coauthor on labor earnings, total earnings, patents and citation-weighted patents. It then describes our main results and a series of robustness checks.

A. Research Design

We want to build the counterfactual of compensation and patent production for (real) survivor inventors, had they not experienced the premature death of a co-inventor. Two main challenges arise to identify this causal effect. First, the real survivor inventors are on a different earnings and patent trajectory than the full population of inventors. To address this challenge, we

---

[16]Assignees are the legal patent holders and are typically the employers of the inventors on the patents.

[17]Similar results are obtained when considering other application years as the year of reference. Appendix Table A14 documents that many teams span more than one EIN, which means they most likely cross firm boundaries.

use the control group of placebo survivor inventors described in Section II in a difference-in-differences research design. Second, death may not be exogenous to collaboration patterns.[18] We show that the estimated causal effects of co-inventor death are significant only after the year of death, which alleviates this concern.

Figure 1 confirms non-parametrically that the real and placebo survivor inventors are on similar earnings and patent trajectories before the time of co-inventor death and sharply differ afterward.[19] This bolsters the validity of the research design, especially given that our match algorithm did not use any information on survivor inventors. Real and placebo survivors have similar levels of total earnings before death, but placebo survivors have higher labor earnings than the real survivors before death, indicating that real survivors have a higher share of their total earnings in the form of non-labor earnings . The difference in labor earnings appears roughly constant, at around $2,500 (about 2% of labor earnings). In our regression framework, we use individual fixed effects to absorb this difference.

Figure 1 shows that the earnings profile of survivor inventors flattens out after the time of death, even for the placebo survivor inventors. This may be due to curvature in the age profile of earnings, year fixed effects, or mechanical effects induced by the construction of the sample of survivors. Citations are declining over time, probably primarily due to truncation (patents applied for and granted near the end of our sample do not have the opportunity of being cited). Our regression framework takes all of these effects into account.

---

[18]We cannot think of very convincing examples of why this could be the case, but perhaps a particularly bad collaboration may result in an inventor's death. For a discussion of how pre-trends can be interpreted as anticipation rather than endogeneity of treatment, seeMalani and Reif (2015) .

[19]The figure plots the raw data, without imposing that mean outcomes in the treatment and control groups should be equal prior to death.

Figure 1 offers a transparent depiction of the data and is useful in gauging the magnitude of the causal effect of co-inventor death on total earnings, labor earnings and forward adjusted citations. However, it is not well suited to a precise estimation of the causal effect - since covariates like age are not perfectly balanced across treated and control groups - nor to robust inference. Two types of clusters are important to take into account for inference: even after controlling for a battery of fixed effects, there may be serial correlation in an inventor's outcomes over time and the outcomes of inventors linked to the same deceased may be correlated. We cluster standard errors at the level of the deceased inventors, which takes into account both forms of clustering. [20] [21]

B. Regression Framework

In order to study the dynamics of the effect, while at the same time probing the validity of the research design by testing whether there appears to be any effect of losing a co-inventor before the event actually occurs, we use a panel data model based on five elements, whose relevance has been discussed in the previous subsection. First, we include a full set of leads and lags around the co-inventor death for real survivor inventors (L_{it}^{Real}). The predictive effects associated with these leads and lags are denoted $\{\beta^{Real}(k)\}_{k=-9}^{9}$, where k denotes time relative to death.[22] If the identification assumption described

[20] We are close to observing the population of patent inventors who passed away prematurely between 1996 and 2012. Therefore, we interpret our standard errors with respect to their superpopulation. In Appendix Table B12, we use the coupled bootstrap procedure of ? to estimate standard errors taking into account the matching step.

[21] We are close to observing the population of patent inventors who passed away prematurely between 1996 and 2012. Therefore, we interpret our standard errors with respect to their superpopulation. In Appendix Table B12, we use the coupled bootstrap procedure of ? to estimate standard errors taking into account the matching step

[22] We drop observations where k is below -9 or above +9 because there are too few observations far away from death and the coefficients on these leads and lags are therefore

Panel A. Survivor Inventor's Total Earnings
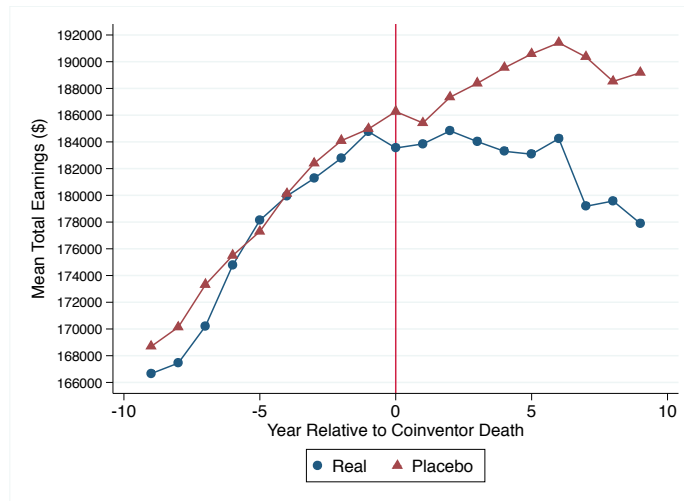
Panel B. Survivor Inventor's Labor Earnings

Figure 1. Path of Outcomes Around Co-inventor Death

imprecisely estimated. Results are qualitatively similar when all observations are kept.

Panel C. Survivor Inventor's Adjusted Forward Citations Received for Patents Applied in Year

Figure 1. Path of Outcomes Around Co-inventor Death(continued)
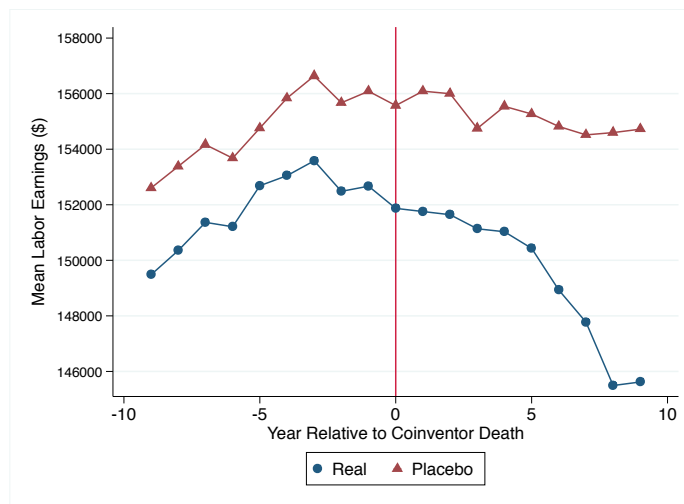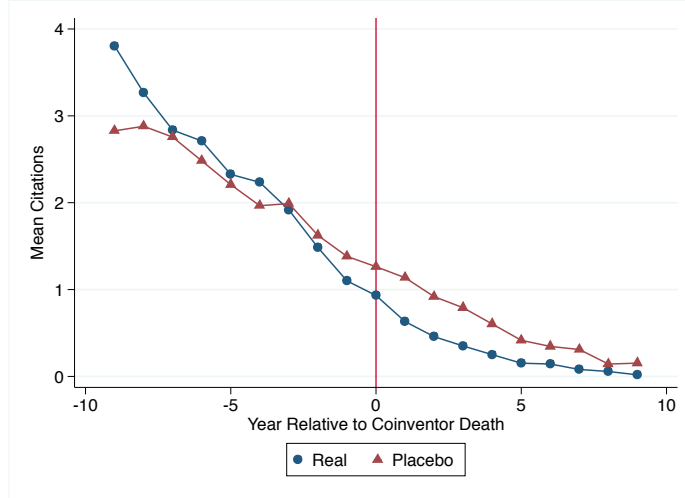
Notes: Panels A to C of this figure show the path of mean total earnings, labor earnings and citations for real and placebo survivor inventors around the year of co-inventor death. The sample includes all real and placebo survivor inventors in a 9-year window around the year of co-inventor death, i.e. inventor-year observations are dropped when the lead or lag relative to co-inventor death is above 9 years. The unbalanced nature of this panel is the same for real and placebo inventors. Appendix Figure B2 shows that the results are similar on a balanced sample. Dollar amounts are reported in 2012 dollars. Refer to Section II.B for more details on the sample and to Section II.C for more details on the outcome variables.

below holds, $\beta^{Real}(k)$ denotes the causal effect of co-inventor death on the outcome of interest k years after death. Second, we use a full set of leads and lags around co-inventor death that is common to both real and placebo survivors (L_{it}^{All}) - the corresponding predictive effects are denoted $\{\beta^{All}(k)\}_{k=-9}^{9}$. Lastly, we introduce three distinct sets of fixed effects: age fixed effects (a_{it}), year fixed effects (γ_t) and individual fixed effects (α_i).

We assume separability[23] and specify the conditional expectation functions as follows:

$$E[Y_{it}|L_{it}^{Real}, L_{it}^{All}, a_{it}, t, i] = f(L_{it}^{Real}) + f(L_{it}^{All}) + g(a_{it}) + \gamma(t) + \alpha_i$$

We then estimate the model with a full set of fixed effects by OLS:[24]

---

[23]The results are qualitatively similar when interacting age and year fixed effects.

[24]We exclude observations with inventors below the age of 25 or above the age of 70

$$(1) \quad Y_{it} = \sum_{k=-9}^{9} \beta_k^{Real} 1_{\{L_{it}^{Real}=k\}} + \sum_{k=-9}^{9} \beta_k^{All} 1_{\{L_{it}^{All}=k\}}$$
$$+ \sum_{j=25}^{70} \lambda_j 1_{\{age_{it}=j\}} + \sum_{m=1999}^{2012} \gamma_m 1_{\{t=m\}} + \alpha_i + \epsilon_{it}$$

The main difference between our specification and the specifications used in the existing literature relying on premature deaths for identification is that we include a set of leads and lags around death that is common to both real and placebo survivors (L_{it}^{All}), in addition to the set of leads and lags around co-inventor death for the real survivors (L_{it}^{Real}). This application of the standard difference-in-differences estimator[25] to our setting addresses the concern that age, year and individual fixed effects may not fully account for trends in life-time earnings and patents around co-inventor death. An inventor

from the sample to reduce variance, but the results are similar when these observations are included. When the dependent variable is citation or patent counts, we use a Poisson estimator, with QMLE standard errors clustered at the deceased-inventor level. The Poisson estimator with individual fixed effects fails to converge in our sample, therefore we report results without individual fixed effects and, as a robustness check, we run the same specifications with a negative binomial estimator with fixed effects. Note that we use quasi-maximum likelihood methods, therefore we obtain consistent estimates with Poisson even without imposing that the mean should be equal to the variance and even with non-integer data (for a formal reference, see**?**. Also note that these specifications, whether with OLS or Poisson, suffer from the standard collinearity between year, age and individual fixed effects. We drop two of the age fixed effects, as is standard practice. This does not affect our estimates of β_k^{Real}, which are the estimates of interest. Our econometrics appendix, Appendix E, offers an in-depth discussion of these issues.

[25]In the standard difference-in-differences estimator, treatment occurs at only one point in time and the regression includes a $Treated$ dummy for the treatment group, a $Treated \times Post$ dummy turning to one after treatment for the treated, and a $Post$ dummy common to both the treated and control groups. In our setting, where co-inventors death are staggered over time, L_{it}^{All} plays a role analogous to the $Post$ dummy and L_{it}^{Real} plays a role analogous to the $Treated \times Post$ dummy. Using our notation for point estimates in specification (2), the standard difference-in-differences specification is:

$$Y_{it} = \alpha Treated_i + \beta^{All} Post_{it} + \beta^{Real} Treated \times Post + \epsilon_{it}$$

Note that in our research design, the matching step creates a situation where the placebo survivors inherit the counterfactual year of death associated with their placebo deceased inventor (and the corresponding real deceased inventor).

must necessarily have invented a patent before the year of (real or placebo) co-inventor death and is more likely to have been employed at that time, even conditional on a large set of fixed effects. Therefore, the construction of the sample of survivor inventors might mechanically induce a bias that the fixed effects do not fully address, and indeed we find that the set of leads and lags L_{it}^{All} has substantial predictive power for certain outcomes like employment. Intuitively, the leads and lags that are common to both real and placebo survivors (L_{it}^{All}) capture the mechanical effects, while the leads and lags that are specific to the real survivors (L_{it}^{Real}) capture the causal effect of co-inventor death.

Formally, if $E[1_{\{L_{it}^{All}=k\}}\epsilon_{it}|L_{it}^{Real}, L_{it}^{All}, a_{it}, t, i] = 0 \ \forall(t,k)$, then $\beta^{Real}(k)$ gives the causal effect of co-inventor death on the outcome of interest k years after death. Appendix D formally derives what is identified in this model and how the predictive effects $\{\beta^{Real}(k)\}_{k=-9}^{9}$ can be used to probe the validity of the research design and identify causal effects. It also compares our specification to those commonly used in the literature using premature deaths for identification.

In the next subsection, we use specification (1) to confirm the validity of the research design and study the dynamics of the effect. To summarize the results and discuss magnitudes, we employ a second specification, with a dummy turning to one after the time of co-inventor death for real survivor inventors ($AfterDeath_{it}^{Real}$) and another dummy turning to one after the time of co-inventor death for both real and placebo survivor inventors ($AfterDeath_{it}^{All}$). Under our identification assumption, β^{Real} gives the average causal effect of death.[26] This specification is as follows:

---

[26]We have relatively more deaths occurring later in our sample and, as a result, β^{Real} gives more weight to the causal effects of death in the short-run after death and less weight

24

$$(2) \quad \begin{aligned} Y_{it} = {}& \beta^{Real} AfterDeath_{it}^{Real} + \beta^{All} AfterDeath_{it}^{All} \\ & + \sum_{j=25}^{70} \lambda_j 1_{\{age_{it}=j\}} + \sum_{m=1999}^{2012} \gamma_m 1_{\{t=m\}} + \alpha_i + \epsilon_{it} \end{aligned}$$

C. Results

Figure 2 reports the point estimates and 95% confidence interval for the coefficients β_k^{Real}, obtained from specification (1). Four outcome variables are considered: total earnings, labor earnings, non-labor earnings and citations. The point estimate on the lag turning to one in the year preceding death is normalized to 0 and inference is carried out relative to this lag.[27] We observe no pre-trending for any of the outcome variables, which lends credibility to the research design. The effect of co-inventor death on compensation and patents appears to manifest itself gradually: total earnings, labor earnings, non-labor earnings and citations all start to decline gradually after the death of a co-inventor. In line with the event studies in Figure 1, the nonparametric fixed effects for each lead and lag around death thus indicate that the nature of the effect is a change in the slope of the outcomes, rather than a level shift, and that co-inventor death has effects beyond short-term disruption of team-work. As further discussed in Section IV, the gradual nature of the effect is consistent with the view that co-inventor death impedes future co-invention activities: innovation is a stochastic process and the placebo survivors gradually outperform the real survivors.

The magnitude of the effects is large. Eight years after the time of co-

to long-run effects. All results in the paper are about the average treatment effect on the treated.

[27]The full set of leads and lags L_{it}^{Real} always sum up to one for the survivor inventors and our specification includes individual fixed effects, therefore one of the leads and lags must be "normalized" to one. Appendix D discusses this standard normalization more formally.

inventor death, the real survivor inventors' total earnings are $7,000 lower (4% of mean total earnings in the sample of survivors), their labor earnings are about $5,800 lower (3.8% of mean labor earnings in the sample of survivors) and their citation-weighted patent production is 15% lower than it would have been had they not experienced the premature death of a co-inventor.[28] About 80% of the total decline in earnings is due to a decline in labor earnings. We formally test the hypotheses the point estimates are all the same before [29] death, but we can after death.[30]

In order to reduce noise, we use specification (2), with a single indicator turning to one after the year of co-inventor death for real survivor inventors. The results are reported in Table 3. We use thresholds corresponding to the extensive margin, the 25th, 50th and 75th percentiles of the total earnings and labor earnings distributions to characterize heterogeneity in the effect across the income distribution.

Table 3 shows large and statistically significant coefficients β^{Real} for all outcome variables, consistent with the dynamic specifications reported in Fig-

---

[28]The magnitude of the decline in citation-weighted patents is in line with the literature on peer effects in science. In life sciences,Azoulay, Graff Zivin and Wang (2010) find that collaborators experience a 8% decline in quality-adjusted publications after the death of a "star" scientist. Oettl (2012), who also studies "star" scientists, finds a corresponding decline of 16% in immunology. Based on the dismissal of Jewish scientists by the Nazi government, Waldinger (2011) shows that losing a coauthor of average quality reduces the average researcher's publication record by 13% in physics and 16.5% in chemistry.

[29]Bell et al. (2016) conduct event studies of inventor labor and non-labor earnings around the time of patent application and find that inventors' returns to innovation materialize gradually around the time of patent application in the form of both labor and non-labor earnings.

[30]In Appendix Tables B5 and B6, we show the raw event study and the point estimates from the full dynamic specification for the number of patents. The results are similar to those for citation-weighted patents.

Panel A. Survivor Inventor's Total Earnings
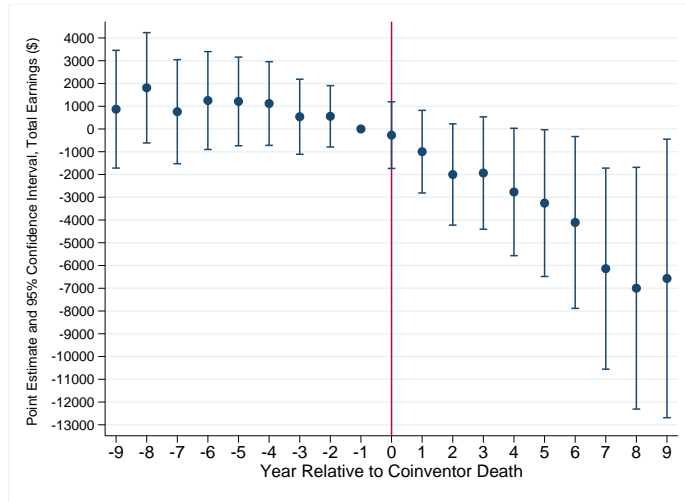
Panel B. Survivor Inventor's Labor Earnings
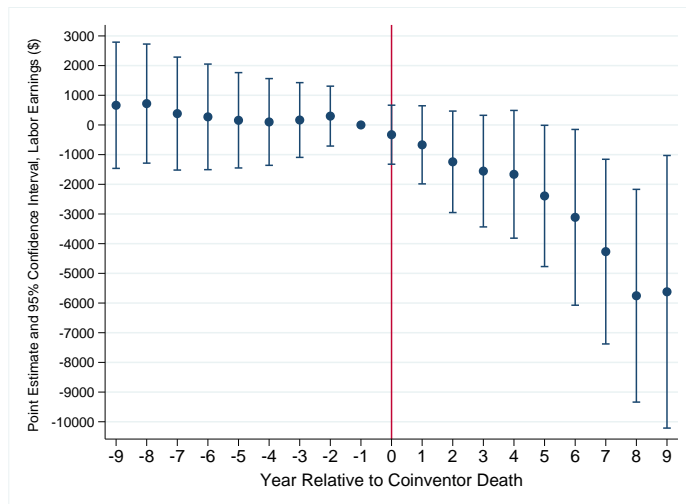
Figure 2. Dynamic Causal Effects of Co-Inventor Death

Panel C. Survivor Inventor's Non-Labor Earnings
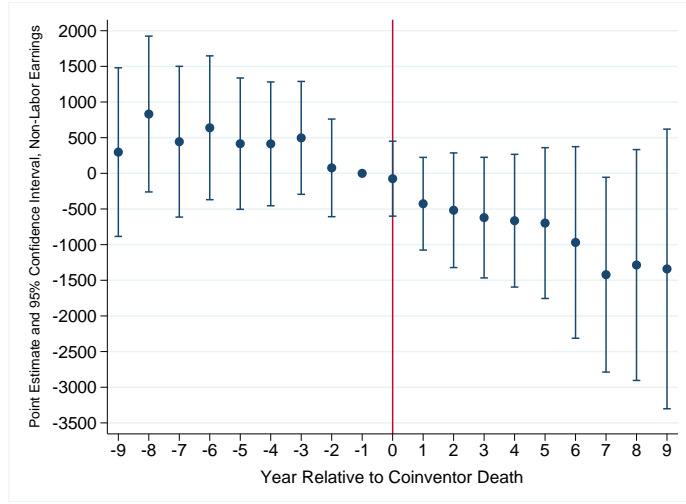
Panel D. Survivor Inventor's Adjusted Forward Citations Received on Patents Applied For in Year
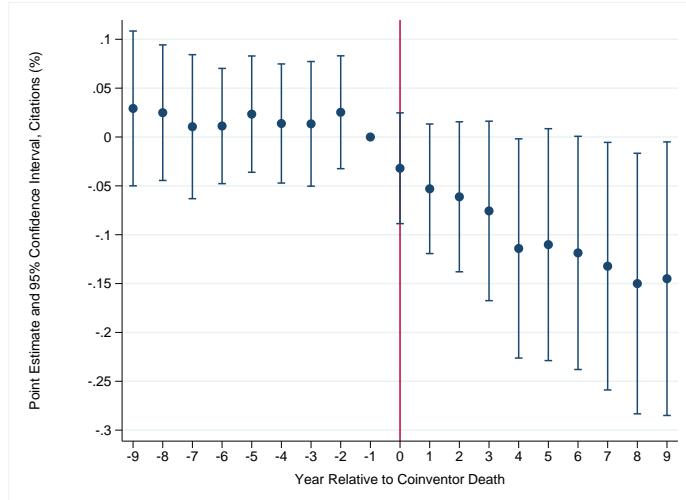
Figure 2. Dynamic Causal Effects of Co-inventor Death *(continued)*

Notes:Panels A to D of this figure shows the estimated β_k^{Real} coefficients from specification (1) for four outcome variables. Standard errors are clustered around the deceased inventors. Under the identification assumption described in Section III.B, β_k^{Real} gives the causal effect of co-inventor death in year k relative to co-inventor death. In panel D, the variable is the count of forward citations received on patents the survivor applied for in a given year. Therefore, this variable reflects the timing and quality of patent applications by the survivor, not the timing of citations. Adjusted forward citations are winsorized at the 0.1% level. Dollar amounts are reported in 2012 dollars. The sample includes all real and placebo survivor inventors in a 9-year window around the year of co-inventor death, i.e. inventor-year observations are dropped when the lead or lag relative to co-inventor death is above 9 years. The unbalanced nature of this panel is the same for real and placebo inventors. Appendix Table B6 shows that the results are similar on a balanced panel. For more details on the outcome variables, refer to Section II.C.

-ure 2. The effect exists across the distribution of total earnings, and it seems larger in lower quantiles - a finding we will probe further in Section IV.Interestingly, β^{All} is significant for two outcomes: non-labor earnings and the extensive margin of labor earnings. The point estimates are large in magnitude relative to the point estimates for β^{Real}, which shows that controlling for mechanical patterns is important to avoid bias, even when age, year and individual fixed effects are included. Panel C of Table 3 shows that co-inventor death has large and significant effects for both the quantity of quality of patents produced by survivor inventors.[31]

D. Additional Results and Robustness Checks

Balanced Panel. We have confirmed that our results are robust to restricting attention to a balanced panel, focusing on survivors whose associated deceased passed away between 2003 and 2008 and considering a four-year window around death for each of these survivors. The results are presented in Appendix Table B6 and are similar to the results using the unbalanced panel.

Long-Term Persistence. The finding that co-inventor death has a lon--glasting effect is a striking result of this paper. Appendix Table B3 confirms

---

[31]The results for β^{Real} reported in Table 3 are similar when running the following specification, which replaces $AfterDeath_{it}^{All}$ in specification (2) with a full set of leads and lags around death (L_{it}^{All}):

$$Y_{it} = \beta^{Real} AfterDeath_{it}^{Real} + \sum_{k=-9}^{9} \beta_k^{All} 1_{\{L_{it}^{All}=k\}} + \sum_{j=25}^{70} \lambda_j 1_{\{age_{it}=j\}} + \sum_{m=1999}^{2012} \gamma_m 1_{\{t=m\}} + \alpha_i + \epsilon_{it}$$

We have also checked that the results obtained with the Poisson estimator for count data are qualitatively similar when using OLS instead.

Table 3—Causal Effects of Co-inventor Death

Panel A. Survivor Inventor's Total Earnings and Non-Labor Earnings

	Total Earnings	>p25	>p50	>p75	Non-Labor Earnings
$AfterDeath^{Real}$	-3,873	-0.01531	-0.0107	-0.00772	-1,199
s.e.	(910)	(0.00434)	(0.00457)	(0.0039)	(498)
$AfterDeath^{All}$	- 223	0.00036	0.00066	-0.00068	651
s.e.	(537)	(0.00285)	(0.00314)	(0.00297)	(378)
Age and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes
# Observations	325,726	325,726	325,726	325,726	325,726
# Survivors	27,500	27,500	27,500	27,500	27,500
# Deceased	9,428	9,428	9,428	9,428	9,428
Estimator	OLS	OLS	OLS	OLS	OLS

Panel B. Survivor Inventor's Labor Earnings

	Labor Earnings	>0	>p25	>p50	>p75
$AfterDeath^{Real}$	-2,715	-0.00913	-0.01039	-0.007203	-0.00638
s.e.	(706)	(0.00315)	(0.00411)	(0.0037)	(0.00342)
$AfterDeath^{All}$	-38	-0.0051	-0.00259	-0.00066	0.00127
s.e.	(480)	(0.00221)	(0.00295)	(0.00322)	(0.003)
Age and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes
# Observations	325,726	325,726	325,726	325,726	325,726
# Survivors	27,500	27,500	27,500	27,500	27,500
# Deceased	9,428	9,428	9,428	9,428	9,428
Estimator	OLS	OLS	OLS	OLS	OLS

Table 3—Causal Effects of Co-inventor Death *(continued)*

Panel C: Survivor Inventor's Patent Applications and Forward Citations

	Patent Count	Citation Count	Count of Patents with No Citations	Count of Patents in Top 5% of Citations
$AfterDeath^{Real}$	-0.09121	-0.09024	-0.07656	-0.02182
s.e.	(0.02063)	(0.02326)	(0.0217)	(0.00789)
$AfterDeath^{All}$	0.00055	0.04084	0.00325	0.00455
s.e.	(0.01776)	(0.03016)	(0.02662)	(0.00554)
Age and Year Fixed Effects	Yes	Yes	Yes	Yes
Individual Fixed Effects	No	No	No	No
# Observations	325,726	325,726	325,726	325,726
# Survivors	27,500	27,500	27,500	27,500
# Deceased	9,428	9,428	9,428	9,428
Estimator	Poisson	Poisson	Poisson	Poisson

Notes:Panels A, B and C report the estimated coefficients β^{Real} and β^{All} from specification (2) for a range of outcome variables. Several outcomes in Panels A and B are indicator variables equal to one when the inventor's earnings are above a given quantile of the earnings distribution. Panel C does not include individual fixed effects because the Poisson estimator with individual fixed effects did not converge for several of the citation outcomes. Appendix Table B10 shows that the results are similar with individual fixed effects, using a negative binomial estimator. The four citation outcome variables in Panel C are as follows: (1) patent count is the number of patents the survivor inventor applied for in a given year; (2) citation count is the number of forward citations received on patents that the survivor applied for in a given year (therefore, this variable reflects the timing and quality of patent applications by the survivor, not the timing of citations); (3) the count of patents with no citations is the number of patents that the survivor inventor applied for in a given year and that have never been cited as of December 2012; (4) the count of patents in the top 5% of citations is the number of patents the survivor inventor applied for in a given year that were in the top 5% of the citation distribution, where the distribution is computed based on all patents that were cited, applied for in the same year and in the same technology class (we aggregate USPC classes into six main technology classes, as is common in the literature). The sample includes all real and placebo survivor inventors in a 9-year window around the year of co-inventor death, i.e. inventor-year observations are dropped when the lead or lag relative to co-inventor death is more than 9 years. The unbalanced nature of this panel is the same for real and placebo inventors. Appendix Table B6 shows that the results are similar on a balanced panel. Standard errors are clustered around the deceased inventors.

that the effect becomes larger over time in a statistically significant way, using a specification with an indicator turning to one for observations more than four years after death (which reduces the noise reflected by the standard errors shown on Figure 2). A potential concern when studying the dynamics of the effect is related to how unbalanced the panel is with respect to years before and after the death of the co-inventor. For example, recent deaths have many pre-

death observations but few post-death observations while the opposite holds for early deaths in the sample. The dynamic specification can confound true dynamics due to the changing composition of the sample.[32] To address this issue, Appendix Figure B2 shows the path of total earnings for real and placebo survivor inventors experiencing death of their co-inventor between 2003 and 2005. This allows us to track the same individuals over time and confirms that the effect of coauthor death is indeed gradual and long-lasting. The regression results are presented in Appendix Table B4 and are qualitatively similar to the findings reported in Figure 2. In Section V, we show that the long-term persistence of co-inventor death is likely explained by the fact that team-specific capital is accumulated over time, as a collaboration unfolds, and it is therefore difficult for inventors to reconstitute it and come back to trend.

Explaining why the effect appears gradually. The slow dissipation of rents from previous collaborations is a potentially important reason why the causal effect of co-inventor death manifests itself gradually, as shown by the changes in slopes on the various panels of Figure 2. Intuitively, innovation can be viewed as a Poisson process with an inventor-specific rate of success λ^i. Assume that when an inventor loses a co-inventor, their probability of successful innovation drops discontinuously to $s \cdot \lambda^i$, with $s < 1$, because the loss of a co-inventor makes them less productive. Then, the path of their innovation outcomes (patents and citations) and returns to innovation (earnings) will not drop suddenly but, rather, will feature a gradual decline because the

---

[32]For example, it could be that inventors who experience death of a coauthor earlier in the sample are of higher ability than inventors who experience death of a coauthor later in the sample, which would manifest itself as larger long-run than short-run effects of death that are entirely due to changing sample composition rather than dynamic cumulative impacts. Similarly, one could imagine that earlier deaths in the sample had a bigger impact than later deaths but the impacts are constant following death: again, this would induce larger long-run than short-run effects, resulting from changing composition rather than dynamic cumulative impacts.

rents from previous collaboration dissipate slowly. For instance, the survivor may be much less productive in the year immediately following co-inventor death, but it typically takes more than a year to successfully invent and file a new patent. Therefore the measured difference in number of patents between the real and placebo survivors will be small in the first year, although the underlying difference in innovation rates may be highest then.[33] We show the relevance of this channel by documenting in Appendix Tables B1 and B2 that the effect is gradual primarily in technology categories with a slow "speed of patenting".[34] In technology categories where it takes less time to invent, the effect of co-inventor death is still long-lasting (we investigate the reason why in Section V) but much less gradual than in technology categories where it takes a longer time. In our preferred specification, we proxy for the speed of patenting in a technology class using a citation lag measure, the average number of years between the application dates of the citing and cited patents. We discuss in Appendix B robustness checks using alternative proxies.

Additional Robustness Checks. Appendix B reports a series of additional robustness checks showing that the results do not stem from lingering health conditions (Appendix Figure B3 and Table B7), are similar with a propensity-score reweighting strategy (Appendix Figure B4 and Table B8), are robust to considering alternative measures of citations (Appendix Tables B9 and B10), and are robust across technology classes (Appendix Table B11). We also show that the results preserve strong statistical significance with an inference procedure taking into account the matching step (Appendix Table

---

[33]The same logic applies to income differences, assuming that inventors are rewarded for their successful innovations, in line with the evidence from earnings event studies around patent application presented in Toivanen and Väänänen (2012) or Finland, Depalo, Addario and Lucia (2014) for Italy, and Bell et al. (2016) for the US.

[34]An alternative explanation for the gradual nature of the effect is convex returns to team building, which we test and reject in Section V.

B12) and are similar when using log transformations and non-winsorized variables (Appendix Tables B13 and B14).

III.   Does Team-Specific Capital Matter?

In this section, we show that the long-lasting decline in earnings and citations caused by the premature death of a co-inventor stems from the fact that the survivor lost a co-inventor with whom they were collaborating extensively. We first rule out alternative mechanisms that are not specific to the team. Second, we show that, within the team, the effect is not driven by asymmetric top-down spillovers from unusually high-achieving deceased inventors. Third, we demonstrate that the intensity of the collaboration between the deceased and the survivor inventors prior to death is an important predictor of the magnitude of the effect. Fourth, we document that the majority of the effect results from the fact that the survivor can no longer co-invent with the deceased: when considering only patents that were invented by the survivor without the deceased, the effect becomes much smaller. Together, these facts indicate that team-specific capital is likely to be a central mechanism explaining the findings from Section III.

A.   Ruling Out Mechanisms That Are Not Specific to the Team

Firm disruption and network effects. We first investigate whether disruption of the firm or diffuse network effects are important channels. To do so, we consider the groups of real and placebo coworkers and second-degree connections.[35]

---

[35]The coworkers are the inventors who were in the same firm as the deceased in the year prior to death. The second-degree connection are the co-inventors of the co-inventors of the deceased. Refer to Section II for more details about the definition of these groups and the construction of the sample.

Panel A. Coworker's Total Earnings
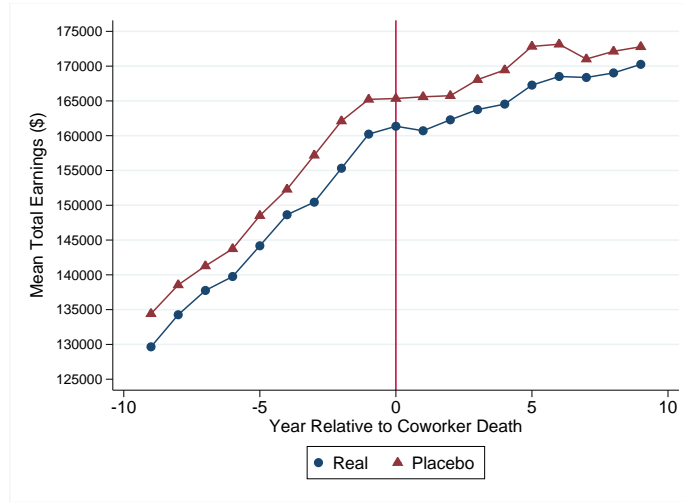
Panel B. Second-degree Connections' Total Earnings
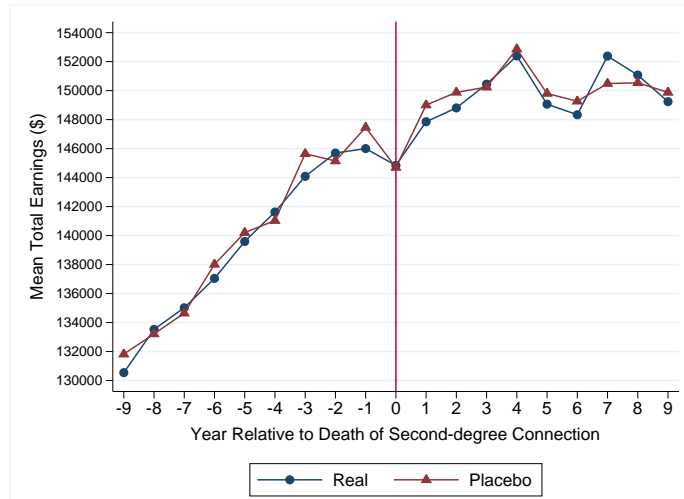
Figure 3. Path of Outcomes for Coworkers and Second-Degree Connections
Around Death

Notes: This figure shows the path of mean total earnings for real and placebo coworkers as well as for real and placebo second-degree connections around the year of death of their associated deceased. The sample includes all real and placebo inventors in a 9-year window around the year of co-inventor death, i.e. inventor-year observations are dropped when the lead or lag relative to co-inventor death is above 9 years. The unbalanced nature of this panel is the same for real and placebo inventors. Dollar amounts are reported in 2012 dollars. Refer to section II.B for more details on the sample and to section II.C for more details on the outcome variables.

Figure 3 shows that the real and placebo coworkers and the real and placebo second-degree connections follow similar earnings paths both before and after the year of death of their associated deceased.[36] Appendix Figure C1 shows similar results for the paths of labor earnings and citations. This stands in sharp contrast with the diverging paths of real and placebo survivors after co-inventor death, presented in Figure 1.

Appendix Table C1 reports the results obtained from specification (2) and shows that the premature death of an inventor has no significant negative effect on their coworkers and second-degree connections. The point estimates for the various outcome variables are generally one or two orders of magnitude smaller than the point estimates obtained for the direct co-inventors and are relatively precisely estimated.

We find small and significant *positive* effects of an inventor's death on their coworkers' likelihood of being employed as well as on their patent and citation counts. Therefore, the large negative effect on the direct co-inventors of the deceased documented in Section III do not result from the disruption of the firm or the R&D lab following an inventor's death.[37] The positive effect on coworkers may result from substitutability between inventors at the same firm: an inventor's earnings and patent production might rise after the death of a coworker because it increases this inventor's chance of being promoted and their access to resources within the firm.[38] We have checked that similar

---

[36]The path of earnings for coworkers and second-degree connections - whether real or placebo - exhibits strong curvature around the time of (real or placebo) death. This curvature is partly captured by year and age effects. It also results from the fact that we impose that the coworkers should be employed in the year preceding death and that the second-degree connection should have co-invented with the survivors prior to death.

[37]We provide additional evidence confirming this fact by showing that the effect persists for co-inventors located in different firms (as proxied for by EINs) at the time of death (Appendix Table C17) and that the magnitude of the effect is not correlated with firm size (Appendix Table C22).

[38]Further exploration of the mechanism at play for coworkers is beyond the scope of this

results hold when we restrict attention to coworkers that were in the same commuting zone as the deceased in the year prior to death: see Appendix Table C3.

For the second-degree connections, we find no statistically significant effect on any of the outcomes. The point estimates are close to zero and we can reject at the 5% confidence level any effect of a magnitude larger than one half of the effect documented for the direct co-inventors. This evidence provides a test of competing models of strategic interactions in networks. If the dominant force is a substitution effect as in Jackson and Wolinsky (1996) , then we should find that the second-degree connections benefit from the death. But if strategic complementarities dominate as in Bramoullé, Kranton and D'amours (2014) , then the death should negatively affect the second-degree connections. Our finding that, on net, the effect on second-degree connections is negligible means that network effects are not first-order, as opposed to the direct impact on co-inventors.

Therefore, we can rule out firm disruption and network effects as primary mechanisms explaining the effect documented in Section III.[39] Moreover, the analysis of the effect on coworkers and second-degree connections generates new insights about the innovation production function: the results suggest that inventors within a firm are substitutable while there is no strong complementarity or substitutability patterns between inventors who are two nodes

---

paper, but our results are consistent with those obtained in parallel work by Jäger (2016) , who studies small firms in Germany rather than the population of inventors, as we do.

[39]We have also constructed a "citation network" of inventors who cited the deceased before their death but who were not among their direct co-inventors, second-degree connections or coworkers. We do not find evidence of statistically significant negative effects. These results are not surprising, given how diffuse citation networks are, but they establish that the effect is not driven by linkages in idea space. These results are available from the authors upon request.

away in the co-invention network.[40]

Loss of firm-specific capital. To rule out that the effect is driven by the loss of traditional firm-specific capital, we show that the effect persists even when inventors were located in different firms at the time of co-inventor death. Since EINs are an imperfect measure of firms, we focus on a subsample where either the deceased or the survivor is located in an academic EIN, and the other works for an EIN in the private sector, which unambiguously guarantees that they were indeed working for different entities. Considering such collaborations, we report in Appendix Table C13 that the effect persists and is similar in magnitude to the effect in the full sample. We conduct a series of related exercises in Appendix C. First, as another way of ruling out the loss of traditional firm-specific capital as an important driver of the effect, we show that the effect is of a similar magnitude for inventors who do not switch EINs after co-inventor death (Appendix Table C15). Second, we show that the effect persists for inventors located in different EINs and in different commuting zones prior to co-inventor death, suggesting that team-specific capital is not tied to firm or geographic boundaries (Appendix Table C17).[41]

Ruling out other mechanisms. We examine other mechanisms in which team-specific capital plays no role in Appendix C, including the loss of "person-

---

[40]Our quasi-experiment does not deliver insights about general substitution and complementarity patterns in the patent production function or in extended co-inventor networks. Indeed, the reduced-form effects we identify correspond to the idiosyncratic effect of an inventor on their coworkers and second-degree connections. It could be that the production function exhibits strong complementarities between coworkers, and yet that the causal effect of the premature death of an inventor's coworker on this inventor's earnings and patents is a precise zero, simply because this coworker can be replaced. Our analysis shows that co-inventors are a source of specific value for an inventor, in a way that coworkers and second-degree connections are not. See Appendix C for a complete discussion.

[41]The limitations of these additional tests are that, in the first case, we are conditioning on an endogenous outcome and, in the second case, there remains ambiguity about whether different EINs really correspond to different firms. Appendix Table C14 documents heterogeneity in the treatment effect for teams in academia versus the private sector.

specific capital" (the idea that a given inventor may be irreplaceable to anyone who ever collaborated with them, regardless of team dynamics), emotional distress, disruption of current work, and changes in physical inputs available to survivor inventors, among others.

B. Top-Down Spillovers Are Not the Driving Force

As mentioned in Section II, some teams are composed of inventors of similar age and compensation levels, while in others there are large gaps in age and compensation levels between team members. We study whether these patterns are important predictors of the heterogeneity in the average effects documented in Section III. In particular, we want to test whether the effect is driven by the death of "superstar" inventors or, more generally, by inventors of higher ability level than their associated survivors.

To do so, we repeat the estimation of the coefficient of interest, β^{Real} , by using specification (2) in different subsamples of the data. We partition the data depending on the quartile in which the total earnings of the (real and placebo) deceased and the (real and placebo) survivor inventors fall three years before the year of (real and placebo) death. The sample sizes in each subsample are given in Appendix Table C4. This way of inferring relative ability levels can potentially create mean reversion patterns. For instance, it could be that survivor inventors who are in the first quartile of the earnings distribution three years before co-inventor death suffered from temporary shocks and that their earnings tend, on average, to increase afterwards. The use of our control group of placebo survivor inventors is sufficient to alleviate these concerns if the income processes are similar for the real and placebo survivor inventors prior to the death of the co-inventor (i.e. both groups are affected by mean reversion and other such patterns in similar ways). To investigate whether this

is true, we examine the distribution of changes in total earnings for the years before the death of the co-inventor. The difference in this analysis relative to our earlier analysis in Section III is that we now want to ensure that the placebo survivor inventors are an appropriate control group for the distribution of changes in potential outcomes over time, not just for their mean. Appendix Table C5 shows that the distribution of earnings changes is very similar for the real and placebo survivor inventors.[42]

Table 4 reports the results of this analysis with total earnings as the outcome. Three main findings stand out. First, the effect is significant and large in magnitude when the deceased and the survivor are in the same earnings quartile, i.e. are of similar seniority levels. This rejects the hypothesis that the effect documented in Section III is entirely driven by top-down spillovers from "superstar" inventors, because the effect persists for inventors of similar seniority levels. Second, holding constant the earnings quartile of the survivor, the effect is increasing in the earnings quartile of the deceased, showing that co-inventors of a higher seniority level are more difficult to substitute for. In other words, although top-down spillovers are not the entire story, they are very much part of the story. Third, the effect is not significant when the deceased is in a lower earnings quartile than the survivor. Although the point estimates are imprecisely estimated, it suggests that co-inventors of a lower seniority level are not a source of specific value for an inventor. The fact that lower ability team members suffer from the loss of higher ability team members, while in contrast higher ability team members are largely unaffected by the loss of a lower ability peer, could indicate that lower ability inventors extract "rents" from their collaboration with high ability co-inventors. However,

---

[42]We obtain similar results when considering changes of total earnings in levels as well as level or log changes for labor earnings.

this "rent" hypothesis cannot explain the large effect we find for team members of similar ability levels.

Moreover, Appendix Table C7 shows that mechanical patterns (due to mean reversion or other statistical effects) play a very important role. This table shows that there are strong mean-reversion patterns: survivors in the lowest earnings quartile before (placebo) co-inventor death tend to perform better after the year of death, while survivors in the highest earnings quartile before (placebo) co-inventor death tend to perform worse after the year of death. Therefore, year, age and individual fixed effects are not sufficient to account for trends in earnings around the time of co-inventor death and it is important to include the $AfterDeath^{All}$ dummy introduced in specification (2).

We have confirmed the robustness of these results. First, similar results hold with other outcome variables, as shown in Appendix Tables C6 and C7 for labor earnings. Second, we obtain similar findings when we measure relative ability using citations instead of earnings. Panel A of Appendix Table C8 shows these results.[43] Moreover, Panel B of Appendix Table C8 shows that the effect is much larger when the deceased was a "star", in the top 2% of the citation distribution. Our results are therefore consistent with Azoulay, Graff Zivin and Wang (2010): stars have a very large impact on the people they work with. However, we have shown above that the average treatment effect we document in this paper is not driven by stars - it persists in samples that exclude these very high-achieving individuals. Finally, instead of running the analysis in different subsamples as in Table 4, we ran regressions with an

---

[43]A limitation of using relative citations before death is that the survivor and the deceased have often co-invented most of their patents together, therefore relative earnings appear to be a better signal of relative seniority.

interaction between treatment status and the quartile difference or the level difference in the labor earnings levels of the survivor and the deceased, as well as with the age difference between the survivor and the deceased.

Table 4—Heterogeneity by Relative Ability Levels of Co-Inventors

Deceased Earnings Quartile / Survivor Earnings Quartile	1	2	3	4
1	-2,652	-1,301	1,298	902
s.e.	(1,553)	(1,328)	(1,680)	(1,081)
2	-3,573	-2,798	-810	-1,308
s.e.	(2,111)	(1,178)	(1,675)	(1,278)
3	-5,656	-4,151	-3,243	-2,939
s.e.	(2,612)	(1,968)	(1,632)	(2,562)
4	-6,566	-5,132	-4,853	-7,037
s.e.	(3,450)	(2,530)	(2,650)	(3,256)

Notes: This panel reports the estimated coefficient β^{Real} from specification (2), with total earnings of the survivors as the outcome variable, in sixteen subsamples of the data. Each of these subsamples corresponds to a different combination of the total earnings quartiles of the survivor and the deceased. The earnings quartiles are computed three years before death and sample sizes for each subsample are given in Appendix Table C4. Under the identification assumption described in Section III.B, β^{Real} gives the causal effect of co-inventor death on total earnings. For instance, the panel shows that if the survivor and the deceased were both in the lowest quartile of total earnings three years before death, the causal effect of co-inventor death on the survivor was a decline of $2,652 in total earnings. Amounts are reported in 2012 dollars. Standard errors are clustered around the deceased inventors.

C. The Role of Team-Specific Capital: the Effect Is Driven by Close-Knit Teams and Joint Production

Heterogeneity in the treatment effect across team structures and outcomes suggests that team-specific capital drives the effect. First, we show that the effect is much larger in "close-knit" teams, characterized by an intense history of collaboration. Second, we show that the effect on patents is driven by co-invention activities, rather than by knowledge transmission. Finally, we show that the effect is bigger in teams where the survivors were interacting collectively with the deceased, rather than in a series of dyadic interactions.

Heterogeneity by intensity of collaboration. We consider various measures of collaboration intensity between deceased and survivor inventors,

which Table 5 shows vary widely in our sample. Specifically, we use the number and share of patents the survivor inventor co-invented with the deceased, collaboration length (defined as the number of years between the first and last joint patent application between the survivor and the deceased), and collaboration recency (defined as the numbers of years between the death of the co-inventor and the application for the last co-invented patent with the survivor).

To examine whether heterogeneity in collaboration strength predicts heterogeneity in the causal effects, we set up the following specification:

$$
\begin{aligned}
Y_{it} = {} & \beta^{Real} AfterDeath_{it}^{Real} + \eta^{Real} X_i \cdot AfterDeath_{it}^{Real} \\
& + \beta^{All} AfterDeath_{it}^{All} + \eta^{All} X_i \cdot AfterDeath_{it}^{All} \\
& + \sum_{j=25}^{70} \lambda_j 1_{\{age_{it}=j\}} + \sum_{m=1999}^{2012} \gamma_m 1_{\{t=m\}} + \alpha_i + \epsilon_{it}
\end{aligned}
$$

(3)

where X_i is a vector including all variables listed in Table 5, as well as the age of the survivor inventor at the time of death. The vector X_i is demeaned so that the point estimates for β^{Real} and β^{All} are left unaffected.[44]

Table 6 reports the results for the relevant interaction terms. It shows that the various proxies for the intensity of the collaboration between the survivor inventor and the deceased (co-patent share, collaboration length and collaboration recency) are strong predictors of the magnitude of the causal effect of co-inventor death on the various outcomes. Using the standard deviations reported in Table 5 for the various regressors and the magnitude of the causal effects reported in Table 3, we can gauge the magnitude of the predictive effects. A one standard deviation increase in the share of copatents explains 75% of the average effect on total earnings, 78% of the average effect on labor

---

[44]In Appendix Table C18, we report the results by introducing the interaction terms one at a time, with total earnings as the outcome.

Table 5—Collaboration Patterns Between Deceased and Survivor Inventors Before Death

Variable	Sample	Mean	SD	10pc	25pc	50pc	75pc	90pc
# Patents	Real	8.114	17.285	1	1	3	9	18
	Placebo	7.41082	12.757	1	1	3	8	18
# Co-patents	Real	1.702	1.502	1	1	1	2	3
	Placebo	1.6108	1.394	1	1	1	2	3
Co-patent Share	Real	54.61	37.75	7.692	18.75	50	100	100
	Placebo	54.55	37.81	8.33	18.18	50	100	100
Collaboration Length	Real	0.8208	1.7393	0	0	0	1	3
	Placebo	0.7593	1.7050	0	0	0	1	3
Collaboration Recency	Real	6.1125	3.9756	1	3	6	9	12
	Placebo	5.673	4.0078	1	2	5	8	12
# Real Survivors	14,150							
# Placebo Survivors	13,350							

Notes: The variables are defined as follows: (1) # patents is the number of patents of the survivor before co-inventor death; (2) # co-patents is the number of patents co-invented by the survivor and the deceased before co-inventor death; (3) co-patent share is the share of the survivor's patents that were co-invented with the deceased before death; (4) collaboration length is the number of years that elapsed between the first and last joint patent application between the survivor and the deceased; (5) collaboration recency is the number of years that elapsed between the application year for the last patent co-invented by the survivor and the deceased and the year of co-inventor death.

earnings, 70% of the average effect on patent count, and 54% of the average effect on citation count. Similarly, a one standard deviation increase in collaboration length explains 47% of the average effect on total earnings, 33% of the average effect on labor earnings, 46% of the average effect on patents, and 53% of the average effect on citations. Lastly, a one standard deviation increase in collaboration recency explains 45% of the average effect on total earnings, 52% of the average effect of labor earnings, 22% of the average effect on patents, and 21% of the average effect on citations. This indicates that the

Table 6—Heterogeneity by Intensity of Collaboration Between Deceased and Survivors

η^{Real}	Total Earnings	Labor Earnings	Non-Labor Earnings	Patent Count	Citation Count
Co-patent Share	-75.132	-56.669	-17.236	-0.00172	-0.0013
s.e.	(22.552)	(17.164)	(8.342)	(0.00085)	(0.00069)
Collaboration Length	-1,063.253	-523.296	-323.296	-0.0245	-0.02892
s.e.	(405.382)	(228.55)	(118.516)	(0.01072)	(0.01537)
Collaboration Recency	447.921	360.281	110.728	0.00508	0.00482
s.e.	(145.592)	(139.825)	(50.95)	(0.00256)	(0.00266)
# Co-patents	42.163	64.029	20.231	0.0015	0.00127
s.e.	(107.372)	(121.255)	(431.156)	(0.01962)	(0.0124)
# Patents	-49.129	5.022	-60.001	-0.00642	-0.00442
s.e.	(57.941)	(39.44)	(40.223)	(0.00287)	(0.00181)
Survivor's Age at Death	104.78	40.961	50.899	- 0.00243	-0.00323
s.e.	(62.774)	(49.876)	(40.85)	(0.001073)	(0.00129)
Age and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	No	No
# Observations	325,726	325,726	325,726	325,726	325,726
# Survivors	27,500	27,500	27,500	27,500	27,500
# Deceased	9,428	9,428	9,428	9,428	9,428
Estimator	OLS	OLS	OLS	Poisson	Poisson

Notes: This table reports the estimated coefficients in the vector η^{Real} from specification (3). The regressors are defined in the main text as well as in Table 5 and are demeaned so that the point estimates for the average causal effects are identical to Table 3. Standard errors are clustered around the deceased inventors.

effect is driven by the loss of a co-inventor that the survivor was collaborating with extensively.[45]

Joint production. Consistent with the team-specific capital interpretation, we find that the effect of co-inventor death is much larger in the context of joint production. We repeat the analysis of the effect of co-inventor death

---

[45]Our results differ markedly from Azoulay, Graff Zivin and Wang (2010), who do not find collaboration intensity to be predictive of the magnitude of the effect of the death of a superstar on their coauthors. It could be due to the fact that top-down spillovers, which are not the driving force in our data, do not strongly depend on the intensity of collaboration.

on the patents of the survivor, but now we only consider patents that were not co-invented with the deceased.[46]

Table 7 reports that, for the various measures of patent production and citations, we consistently find a significant and negative effect of co-inventor death. Continued interaction with a co-inventor therefore benefits an inventor beyond co-inventions, which is consistent with the view of teams as a vehicle for knowledge transmission. However, the magnitude of the effect on the survivor's patents outside of patents with the deceased is much smaller (around -3%) relative to the effect on the total number of patents of the survivor documented in Table 3 (around -9%). This suggests that the main value of team-specific capital comes in the form of co-inventions and that the effect results from the fact that the survivor can no longer engage in joint projects with the deceased.[47]

Heterogeneity by degree of co-invention overlap. We find that the effect on survivors is larger when they were collectively interacting with the deceased. We average the share of patents in common between all survivors associated with a given deceased as a measure of co-invention overlap in this deceased's set of co-inventors. Appendix Table C9 shows that the effect increases by about 10% for all of our outcomes when the degree of co-invention overlap increases by one standard deviation. This finding suggests that there are negative feedback effects when more collaborators in a given inventor's n-

---

[46]Note that legal requirements impose that all inventors should be listed on a patent, otherwise the patent could be invalidated in court. We can therefore be confident that the patents that do no list the name of the deceased were indeed invented without the active collaboration of the deceased.

[47]Note that our results are very different from Azoulay, Graff Zivin and Wang (2010) , who find that the death of a "star" scientist causes a decline of similar magnitude in scientific publications with and without the deceased. In our setting, the importance of joint production between the deceased and the survivor is consistent with the gradual effect documented in Section III: innovation is a stochastic process and the placebo survivors gradually outperform the real survivors.

Table 7—The Causal Effect of Co-inventor Death On the Survivor Beyond Joint Production

| | Only Considering Patents that Were Not Co-invented With the Deceased | | | |
	Patent Count	Citation Count	Count of Patents with No Citations	Count of Patents in Top 5% of Citations
$AfterDeath^{Real}$	-0.03088	-0.03571	-0.03288	-0.0084
s.e.	(0.01525)	(0.01815)	(0.01525)	(0.00478)
$AfterDeath^{All}$	0.1162	0.08578	0.05763	0.0247
s.e.	(0.05319)	(0.12013)	(0.08136)	(0.02271)
Age and Year Fixed Effects	Yes	Yes	Yes	Yes
Individual Fixed Effects	No	No	No	No
# Observations	325,726	325,726	325,726	325,726
# Survivors	27,500	27,500	27,500	27,500
# Deceased	9,428	9,428	9,428	9,428
Estimator	Poisson	Poisson	Poisson	Poisson

Notes: This table reports the estimated coefficients β^{Real} and β^{All} from specification (2). The four outcome variables are as follows: (1) patent count is the number of patents the survivor inventor applied for in a given year, excluding all patents co-invented with the deceased; (2) citation count is the number of forward citations received on patents that the survivor applied for in a given year, excluding all patents co-invented with the deceased; (3) the count of patents with no citations is the number of patents that the survivor inventor applied for in a given year and that have never been cited as of December 2012, excluding all patents co-invented with the deceased; (4) the count of patents in the top 5% of citations is the number of patents the survivor inventor applied for in a given year that were in the top 5% of the citation distribution, excluding all patents co-invented with the deceased. The sample includes all real and placebo survivor inventors in a 9-year window around the year of co-inventor death, i.e. inventor-year observations are dropped when the lead or lag relative to co-inventor death is more than 9 years. The unbalanced nature of this panel is the same for real and placebo inventors. Standard errors are clustered around the deceased inventors.

-etwork are impacted by an unexpected death

In sum, our results show that team-specific capital is important in an inventor's career because it facilitates co-inventions and - to a lesser extent - knowledge transmission. We have conducted interviews with patent inventors to confirm that this mechanism is plausible.[48] Next, we turn to a closer

---

[48]We spoke with fourteen inventors in small start-ups as well as large R&D labs in Silicon Valley. They pointed out the difficulty of building good collaborative relationships and emphasized the long-lasting nature of successful collaborations, which often continue to exist across firm boundaries.

investigation of the sources of team-specific capital.[49]

IV. What Are the Sources of Team-Specific Capital?

Team-specific capital refers to the notion that, from the perspective of a given inventor, their co-inventors are to some extent irreplaceable. The analysis in Sections III and IV provides direct evidence for the existence and substantial magnitude of team-specific capital. Given this evidence and guided by the literature, we now develop hypotheses regarding how team-specific capital operates and we examine which hypotheses are consistent with the observed heterogeneity in the treatment effect. Although we cannot interpret the results presented in this section as causal, the evidence supports models in which team-specific capital endogenously accumulates over the course of a collaboration.

A. Conceptualization

To help discipline models of technological collaboration, we develop hypotheses to answer two questions regarding the nature of team-specific capital.[50] First, where does team-specific capital come from? We distinguish

---

[49]Appendix C documents other heterogeneity patterns in the effect of co-inventor death - by EIN size, survivor's age, survivor's co-inventor network size and survivor's citizenship status - which are of descriptive interest but are not statistically significant for most outcomes. Appendix C also shows that co-inventor death does not have a strong impact on the probability that an inventor starts new collaborations or changes EINs, except if the inventor was in a small EIN before their co-inventor's death.

[50]An emerging theoretical literature examines how social interactions shape long-term growth. For instance, Lucas and Moll (2014) analyze a model of endogenous growth driven by knowledge transmission through social interactions. They emphasize "top-down" knowledge transmission in line with empirical studies such as Azoulay, Graff Zivin and Wang (2010), who show evidence for diffuse knowledge spillovers in intellectual space from "stars." Our results on team-specific capital point to another force: very circumscribed spillovers in collaboration space for the typical inventor. Thus, our evidence points to specific avenues to pursue in the next generation of growth models with social interactions, taking into account the role of co-invention activities in addition to top-down knowledge transmission.

between the "match" and "experience" views of team-specific capital. Second, how does team-specific capital help increase innovation and earnings? We discuss the role of moral hazard within teams and social dynamics.

The labor economics and team management literatures offer two competing hypotheses about the source of team-specific capital. A first view is that inventors have to incur search costs to find a "good match" among a large set of potential co-inventors. This idea is similar to the notion of "firms as inspection goods" in the literature on firm-specific capital (Jovanovic (1979b)). In this case, team-specific capital is equated with the team's "match quality" and is fixed over time: intuitively, high team-specific capital in a team means that inventors have good collective chemistry. A competing view is that good teams are not "found" but largely "made": team-specific capital accumulates over the course of a collaboration, similar to the notion of "firms as experience goods" of Jovanovic (1979a).

The managerial implications of this debate are clear: if the "experience" view best characterizes team dynamics, then the returns to team-building are high, while improving the matching function between co-inventors may not be first order. The management literature suggests that team-building is effective (e.g Pentland (2012); Fapohunda (2013)), and our setting offers a way of indirectly testing that claim by uncovering properties of team-specific capital.

The literatures on contract theory and the sociology of teams suggest two main channels, moral hazard and social dynamics, through which team-specific capital can increase innovation and earnings. First, teamwork is plagued by moral hazard because team members can imperfectly monitor their respec-

49

tive effort levels.[51] The insight of Holmstrom (1982) is that moral hazard in teams can be solved by the introduction of group incentives.[52] If team-specific capital accumulates during a collaboration, then it provides such group incentives: team members have an extra incentive to exert effort because successful completion of the project increases team-specific capital and hence future innovation and earnings with that team, akin to a bonus. Moreover, team-specific capital makes it more likely that inventors will keep working together, since co-inventors are not easily substituted for, and playing a repeated game reduces moral hazard.[53] We find empirical support for the moral hazard channel in Section V.B.

Second, team-specific capital may help solve communication problems and conflict within the team through social dynamics, in particular for teams with heterogeneous members. The question of whether within-team heterogeneity increases or decreases performance has been studied by a vast literature, with ambiguous predictions.[54] Our setting allows us to examine a related question: does within-team heterogeneity make a team harder to replace? We

---

[51]Free-riding results in suboptimal effort when collectively generating new ideas or when screening and enriching teammates' ideas (Wageman (1995); Diehl and Stroebe (1987); Girotra, Terwiesch and Ulrich (2010))

[52]By relaxing the balanced budget constraint and offering a bonus to the team in case of success or a penalty in case of failure, the principal can ensure that team members will all exert first-best effort levels.

[53]For a formalization of this intuition in the context of innovation, see Stein (2008).

[54]See for instance Easterly and Levine (1997); Alesina and Spolaore (1997); La Ferrara (2007); Miguel, Satyanath and Sergenti (2004); Habyarimana et al. (2007); Hjort (2014) . On the one hand, within-team heterogeneity may be beneficial to a team because combining different perspectives may increase collective creativity (Jackson and Wolinsky (1996); Taylor and Greve (2006)) . On the other hand, within-team heterogeneity may reduce team performance because of preferences (e.g. taste-based discrimination within team, as in Hjort (2014)), because it is easier to sustain credible threats in homogeneous teams (Habyarimana et al. (2007)) and because communication is easier (Stewart and Stasser (1995); Gigone and Hastie (1997); Jehn, Northcraft and Neale (1999)). While the existing literature examines the impact of within-group heterogeneity on the *level* of performance, we focus on *replacement* effects.

find that the treatment effect is increasing in various measures of within-team heterogeneity (income, age, geographic dispersion, etc.). We also show direct evidence that social dynamics are an important component of team-specific capital by testing predictions from Simmel (1908), who theorizes that specific team members constitute the basis for trust in the team.

B.   Match vs. Experience Components of Team-Specific Capital

In this section, we carry out tests of the "match" and "experience" views described in Section V.A. We first show that the testable implications of the "match" view are not borne out in the data and we then provide direct evidence in support of the "experience" view.[55]

The main implication of search-and-matching models is that inventors should suffer less from the loss of their co-inventor, in terms of earnings and patent production, if it is easier for them to find a new match. It should be easier for an inventor in a given technology category to find a new co-inventor if they work in a firm or commuting zone where there is a "thick" market for inventors in that technology category.[56] Lazear (2009) defines market thickness as follows: "a market is thick when the worker receives many offers for a given amount of search effort. [...] Empirical proxies of search costs and offer frequencies include regional population density and occupation concentration

---

[55]Note that the slow dissipation of rents from previous collaboration, documented in Section III, makes it difficult to distinguish between these mechanisms based on the dynamics of the treatment effect alone. Absent slow dissipation, the "match" view would imply a sharp immediate decline in performance, followed by a rebound as survivors re-match. Assuming linear returns to experience, the "experience" view would imply an immediate drop followed by parallel trends. Neither of these patterns is consistent with the data, likely due to slow rent dissipation. Accordingly, we pursue other tests to distinguish between these mechanisms.

[56]We use the 37 "secondary technology categories" defined by the NBER. At this level of aggregation, co-inventors are typically specializing in the same technology categories. There is much more heterogeneity at the level of the 400 technology classes defined by the USPTO.

ratios." To guide our analysis, we test the two key predictions of the formal search-and-matching framework of Jäger (2016) : first, a lower probability of finding a new co-inventor should lead to larger and longer-lasting earnings and patent effects for survivors; second, finding a new team member should be correlated with much smaller negative effects for earnings and patents.[57] Intuitively, under the match view the newly-hired team member immediately becomes a perfect replacement for the inventor that used to be part of that team.

Based on these predictions, we investigate whether inventors suffer less from the loss of their co-inventor, and whether they are able to find new co-inventors more quickly, in environments where the market for inventors similar to them is thick. We build our preferred measure of thickness at the level of the EIN-by-commuting zone, since we have documented earlier that inventors do not change CZs or EINs very frequently (even in response to co-inventor death). For any given inventor, we identify the NBER technology subcategory (Hall et al. (2001)) in which they have obtained most of their patents as of the time of co-inventor death. We then compute how many inventors with similar specialization are in the same EIN-by-commuting zone in the year prior to death. In robustness checks, we show that the results are similar when considering measures at the level of commuting zones and using the density (instead of the number) of inventors with a similar specialization. Ap-

---

[57]Jäger (2016) studies frictions from the point of view of the firm, with a focus on wages, but the model can alternatively be interpreted from the point of view of a team, with a focus on earnings and patents. His equation (11) implies the formal prediction that the speed of re-matching and the speed of earnings and patent adjustments should be identical. Note that if the distribution of quality of potential matches drifts over time, the speed of re-matching and the speed of earnings and patents adjustment do not have to be exactly identical, but the qualitative prediction that finding a new team member should be correlated with smaller negative effects for earnings and patents still holds, and we do not find support for this prediction in the data.

Table 8—Match and Experience Components of Team-Specific Capital

Panel A. Heterogeneity by Number of Inventors in Survivor's NBER Technology Subcategory within

CZ-EIN

	Total Earnings	Labor Earnings	Non-Labor Earnings	Patents	Citations	New Co-inventor
$AfterDeath^{Real}$ $\cdot InventorNumber$ (S.D.)	50.237	-76.711	90.821	0.00912	-0.00512	0.228
s.e.	(45.672)	(85.235)	(151.362)	(0.0304)	(0.0102)	(0.11608)
Age and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	No	No	Yes
Interacted Fixed Effects			EIN-CZ Size Deciles			
# Observations	297,017	297,017	297,017	297,017	297,017	297,017
# Survivors	25,089	25,089	25,089	25,089	25,089	25,089
# Deceased	8,554	8,554	8,554	8,554	8,554	8,554
Estimator	OLS	OLS	OLS	Poisson	Poisson	OLS

Panel B. Heterogeneity by Length of Potential Collaboration

	Total Earnings	Labor Earnings	Non-Labor Earnings	Patent Count	Citation Count
$AfterDeath^{Real}$ $\cdot Potential\ Collaboration\ Length$	-983.345	-619.342	-254.462	-0.0246	-0.0214
s.e.	(363.201)	(221.19)	(148.023)	(0.01118)	(0.01081)
Age and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	No	No
Interacted Controls		Survivor's Age at First Patent and Survivor's Age at Co-Inventor Death			
# Observations	325,726	325,726	325,726	325,726	325,726
# Survivors	27,500	27,500	27,500	27,500	27,500
# Deceased	9,428	9,428	9,428	9,428	9,428
Estimator	OLS	OLS	OLS	Poisson	Poisson

Table 8—Match and Experience Components of Team-Specific Capital *(continued)*

Panel C. Are the Returns to Experience Quadratic?

	Total Earnings	Labor Earnings	Non-Labor Earnings	Patent Count	Citation Count
$AfterDeath^{Real}$ $\cdot Potential\ Collaboration\ Length$	-901.523	-640.212	-280.462	-0.0216	-0.0223
s.e.	(346.538)	(266.754)	(146.838)	(0.00981)	(0.01062)
$AfterDeath^{Real}$ $\cdot (Potential\ Collaboration\ Length)^2$	38.534	-50.211	60.231	0.00145	-0.002012
s.e.	(45.103)	(78.40)	(94.928)	(0.00982)	(0.001524)
Age and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	No	No
Interacted Controls	Survivor's Age at First Patent and Survivor's Age at Co-Inventor Death				
# Observations	325,726	325,726	325,726	325,726	325,726
# Survivors	27,500	27,500	27,500	27,500	27,500
# Deceased	9,428	9,428	9,428	9,428	9,428
Estimator	OLS	OLS	OLS	Poisson	Poisson

Panel D. Heterogeneity by Length of Potential Collaboration and Survivor Age at First Collaboration

	Total Earnings	Labor Earnings	Non-Labor Earnings	Patent Count	Citation Count
$AfterDeath^{Real}$ $\cdot Potential\ Collaboration\ Length$ $\cdot Age\ at\ First\ Collaboration/10$	583.345	380.342	154.462	0.00882	0.00924
s.e.	(272.424)	(190.091)	(132.023)	(0.004027)	(0.004978)
Age and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	No	No
Interacted Controls	Survivor's Age at First Patent, Survivor's Age at Co-Inventor Death, Potential Collaboration Length				
# Observations	325,726	325,726	325,726	325,726	325,726
# Survivors	27,500	27,500	27,500	27,500	27,500
# Deceased	9,428	9,428	9,428	9,428	9,428
Estimator	OLS	OLS	OLS	Poisson	Poisson

Notes: Panel A documents the heterogeneity in the treatment effect depending on the number of inventors in the survivor's technology subcategory, within the inventor's CZ-EIN in the year preceding co-inventor death (denoted "inventor number" in the table and standardized by its standard deviation). The specification is similar to specication (3), except that the interacted controls now include only the number of inventors and EIN-CZ size deciles. Panels B, C and D document heterogeneity in the treatment effect depending on the length of potential collaboration between the survivor and the deceased, which is defined as the number of years between the first joint patent application from the survivor and the deceased and the year of death. Standard errors are clustered around the deceased inventors.

54

-pendix Table D1 shows the distribution of our thickness measures. Panel A of Table 8 shows that local inventor labor market thickness is *not* predictive of the magnitude of the effect of co-inventor death on any of our earnings or patent outcomes. The point estimates are small and relatively precisely estimated. In contrast, local inventor labor market thickness is predictive of the speed at which inventors are able to re-match: survivors are more likely to find new co-inventors if they work in an environment with more inventors similar to them. Appendix Tables D2 and D3 show similar results using alternative proxies for local inventor labor market thickness.[58] Appendix Table D4 shows that our proxy for market thickness becomes predictive of the speed of re-match only when we build it based on the technology subcategory of the inventor, which confirms that our results are not driven by broad trends in the local concentration of inventors. These results are not in line with the predictions from the "match" view of team-specific capital: when the local inventor labor market is thicker, new co-inventors are found faster but the earnings and patent effects are as large as in less thick markets. These results point to the role of experience effects, as if it took time for a new co-inventor to become an adequate substitute for the deceased, on which we offer direct evidence next. The main prediction of the experience view is that the effect of co-inventor death should be increasing in the length of the collaboration between the survivor and the deceased. Testing this prediction poses two challenges. First, the observed length of collaboration in our sample, defined as the number of years between the first and last patent applications co-invented by the survivor and the deceased, is endogenous and could stem from a high

---

[58]In unreported robustness checks, we obtain similar results when defining the thickness measure from the point of view of the technology category specialization of the deceased, which is highly correlated with that of the survivor.

"fixed match quality".[59] For this reason, to isolate the role of experience we use the length of "potential collaboration", defined as the number of years between the first patent application co-invented by the survivor and the deceased and co-inventor death.[60]

The second empirical challenge stems from the collinearity between potential collaboration length and the difference between survivor's age at co-inventor death and survivor's age at first collaboration.[61] The formation of teams is endogenous and, therefore, the age at first collaboration could be correlated with "fixed match quality" (e.g. if inventors who think alike were trained in the same schools and are more likely to meet earlier in life).[62] Because of the collinearity between potential collaboration length and age effects, we cannot control for both age at first collaboration and age at co-inventor death. However, we can introduce related controls for the survivor's lifecycle, thus addressing the possible correlation with match quality: we do so in Appendix Tables D5 and D6 and obtain similar results.[63]

Panel B of Table 8 shows that potential collaboration length is a strong predictor of the magnitude of the treatment effect. The magnitude of the effect approximately doubles with an additional four years of collaboration for the various earnings and patent outcomes. We interpret these results as

---

[59]Indeed, if the (fixed) match quality between two inventors is high, they are likely to collaborate for a longer duration. We have shown in Table 6 that actual length of collaboration is positively and strongly associated with the magnitude of the treatment effect, but by itself this evidence does not help distinguish between the "match" and "experience" views.

[60]Note that with this proxy our results are likely to be biased downward, because the survivor and deceased may have stopped collaborating by the time of co-inventor death.

[61]Indeed, note that with two inventors i and j, $PotentialCollaborationLength_{ij} \equiv YearCoinventorDeath_{ij} - YearFirstCollaboration_{ij} = AgeAtCoinventorDeath_i - AgeAtFirstCollaboration_i$.

[62]Appendix D2 offers a formalization of the notions of "fixed match quality" and "experience effects", as well as an in-depth discussion of how the collinearity between potential collaboration length and age effects is addressed by our set of controls.

[63]Appendix D.2 provides an in-depth discussion of these issues.

providing evidence for large "returns to experience" in teamwork. In Panel C of Table 8, we test whether the returns to experience are linear or quadratic. We find that the quadratic term is not significant and small in magnitude. Linear returns to experience explain why the effect of losing a co-inventor is long-lasting: the real survivors do not catch up with the placebo survivors, even after re-matching.[64]

Finally, in Panel D of Table 8, we implement a simple test for the idea that the experience component of team-specific capital might come from relationship-specific investments. This view predicts that the horizon of collaboration determines the magnitude of the returns to experience. If the survivor meets the deceased later in their career, the horizon of collaboration is likely to be shorter, implying smaller relationship-specific investments and lower returns to experience. In contrast, if returns to experience mechanically result from learning by doing, then survivor's age at the time of first collaboration with the deceased should not be predictive of the magnitude of the returns. We find that survivor's age is in fact a strong predictor, which is consistent with the role of relationship-specific investments. Taken together, our findings suggests that team-specific capital endogenously accumulates over the course of a collaboration and reduces moral hazard by making team members more interdependent, as discussed in Section V.A.

C. Team Structure and Social Dynamics

In this section, we present evidence on how the treatment effect varies depending on team structure and social dynamics. First, we find that all

---

[64]Note that if we had found convex returns to experience, it could have explained why the effect appears gradually over time. We thank a referee for this suggestion. However, returns appear to be linear (in the range of years that we can observe) and we have shown in Section III that the gradual nature of the effect can be explained by the fact that innovation is a stochastic and long-term process.

measures of within-team heterogeneity from Section II.D give similar results: the effect is larger in more heterogeneous teams. For succinctness, we report results using the within-team coefficient of variation as a measure of team heterogeneity, standardized by its standard deviation. To obtain a comprehensive measure of team heterogeneity, we construct an average of the coefficients of variation for age, cumulative forward citations, and labor earnings. Panel A of Table 9 reports the results: a one standard deviation increase in our heterogeneity measure is associated with an increase in magnitude for the treatment effect of about 15% for the various outcomes. Controls interacted with treatment status ensure that these results are not driven by top-down spillovers or lifecycle effects.[65]

This finding is consistent with the idea that team-specific capital may improve teamwork by increasing trust, which is more likely to be lacking in more heterogeneous teams. To provide direct evidence on the role of trust, we use the dynamics of team formation, following Simmel (1908). We consider the case of triads that were "closed" by one of the team members over the course of our sample. A triad is a team composed of three inventors. We say that inventor A "closed" the A-B-C triad if, prior to the first patent application by this triad, A had filed at least one joint patent application with B and, separately, A had also filed at least one joint patent application with C, but B and C had never had any joint patent. Simmel (1908) theorizes that, in such a case, the inventor that closed the triad constitutes the basis for trust in the team, because the social ties between the other two inventors are much looser. We test Simmel (1908)'s hypothesis by identifying triads that were

---

[65]The results are similar when considering single coefficients of variation, instead of their average. In Appendix Table D7, we report a horse race between various within-team heterogeneity measures.

closed over the course of our sample. We build registers of unique dyads and triads of inventors based on taxpayer identifiers and are thus able to identify instances when the triad was closed as well as which inventor closed the triad. We then study heterogeneity in the treatment effect, in the sample of triads that were closed, depending on whether the deceased closed the triad or not. Triadic closure is a relatively common event and, therefore, we retain a sufficient sample size to conduct this exercise. We control for relative ability levels (interacted with the post-death indicator) to ensure that the results are not driven by asymmetric spillovers. Panel B of 9 shows the results: the magnitude of the treatment effect is about 15% to 30% larger when the deceased closed the triad, relative to when they did not. We show the robustness of this result relative to other samples and sets of interacted controls in Appendix Tables D8 and D9, ensuring that these results are not driven by top-down spillovers or lifecycle effects. These results suggest that team-specific capital operates through social dynamics and increased trust between inventors.

In Appendix D, we report a series of additional tests for two other topics often discussed in the literature on teams: we find that the effect is larger when the team is less geographically dispersed (Appendix Table D10) and we find no significant heterogeneity by team size (Appendix Table D11).

Table 9—Heterogeneity by Team Structure

Panel A. Heterogeneity by Degree of Within-Team Heterogeneity

	Total Earnings	Labor Earnings	Non-Labor Earnings	Patent Count	Citation Count
$AfterDeath^{Real} \cdot \frac{\overline{CV}}{SD(\overline{CV})}$	-522.912	-421.242	-126.120	-0.01892	-0.01710
s.e.	(182.320)	(156.21)	(98.231)	(0.00792)	(0.00773)
$AfterDeath^{Real}$	-3,532.106	-2,630.121	-1,045.118	-0.1122	-0.1190
s.e.	(945.234)	(708.136)	(458.824)	(0.02157)	(0.2139)
Age and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	No	No
Interacted Controls	Relative ability level, Survivor's age at co-inventor death				
# Observations	325,726	325,726	325,726	325,726	325,726
# Survivors	27,500	27,500	27,500	27,500	27,500
# Deceased	9,428	9,428	9,428	9,428	9,428
Estimator	OLS	OLS	OLS	Poisson	Poisson

Panel B. Heterogeneity by Triadic Closure

	Total Earnings	Labor Earnings	Non-Labor Earnings	Patent Count	Citation Count
$AfterDeath^{Real}$ $\cdot DeceasedClosedTriad$	-813.313	-787.35	-179.85	-0.01843	-0.021431
s.e.	(387.695)	(339.375)	(105.794)	(0.00875)	(0.009318)
$AfterDeath^{Real}$	-3,750.231	-2,804.214	-1,150.522	-0.10031	-0.09892
s.e.	(1543.21)	(1070.214)	(660.928)	(0.03459)	(0.042583)
Age and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	No	No
Interacted Controls	Relative ability level, Survivor's age at co-inventor death				
# Observations	15,232	15,232	15,232	15,232	15,232
# Survivors	1,360	1,360	1,360	1,360	1,360
# Deceased	680	680	680	680	680
Estimator	OLS	OLS	OLS	Poisson	Poisson

Notes: Panel A reports heterogeneity in the treatment effect depending on the degree of within-team heterogeneity (measured as the average of the within-team coefficients of variation for age, cumulative for-ward citations and labor earnings, standardized by its standard deviation). Panel B uses the sample of "closed triads", defined in the main text in Section V. C., and shows heterogeneity in the treatment effect depending on whether or not the deceased closed the triad. Standard errors are clustered around the deceased inventors.

V. Conclusion

In this paper, we have shown that team-specific capital is an important ingredient of the typical patent inventor's lifecycle earnings and innovation, much like firm-specific capital is crucial for the typical worker (Topel (1991)). We find that a co-inventor's premature death causes a large and long-lasting decline in an inventor's labor earnings (- 3.8% after 8 years), total earnings (- 4% after 8 years) and citation-weighted patents (- 15% after 8 years). Consistent with the team-specific capital interpretation, the effect is larger for more closely-knit teams and primarily applies to co-invention activities with the deceased.

Analysis of heterogeneity in the treatment effect shows that team-specific capital increases over the course of a collaboration, rather than being fixed over time as in search-and-matching models. Moreover, the results of our heterogeneity analysis are in line with the view that team-specific capital improves the ability of a team to innovate through reduced moral hazard (consistent with Holmstrom (1982)) and through increased trust (consistent with Simmel (1908)). Taken together, these findings help discipline models of technological collaboration and, from a managerial perspective, suggest that the returns to team-building are high.

References

Aghion, Philippe, and Peter Howitt. 1992. "A model of growth through creative destruction." *Econometrica*, 60: 323–351.

Agrawal, Ajay, Devesh Kapur, and John McHale. 2008. "How do spatial and social proximity influence knowledge flows? Evidence from patent data." *Journal of urban economics*, 64(2): 258–269.

Alesina, Alberto, and Enrico Spolaore. 1997. "On the number and size of nations." *The Quarterly Journal of Economics*, 112(4): 1027–1056.

Alexander, Lameez, and Daan Van Knippenberg. 2014. "Teams in pursuit of radical innovation: A goal orientation perspective." *Academy of Management Review*, 39(4): 423–438.

Azoulay, Pierre, Joshua S Graff Zivin, and Jialan Wang. 2010. "Superstar extinction." *The Quarterly Journal of Economics*, 125(2): 549–589.

Becker, Gary S. 1975. "Front matter, human capital: a theoretical and empirical analysis, with special reference to education." In *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education, Second Edition.* 22–0. NBER.

Becker, Sascha O, and Hans K Hvide. 2016. "Do entrepreneurs matter?"

Bell, Alex, Raj Chetty, Xavier Jaravel, Neviana Petkova, and John Van Reenen. 2016. "The lifecycle of inventors."

Bennedsen, Morten, Kasper Meisner Nielsen, Francisco Pérez-González, and Daniel Wolfenzon. 2007. "Inside the family firm: The role of families in succession decisions and performance." *The Quarterly Journal of Economics*, 122(2): 647–691.

Borjas, George J, and Kirk B Doran. 2012. "The collapse of the Soviet Union and the productivity of American mathematicians." *The Quarterly Journal of Economics*, 127(3): 1143–1203.

Borjas, George J, and Kirk B Doran. 2015. "Which peers matter? The relative impacts of collaborators, colleagues, and competitors." *Review of Economics and Statistics*, 97(5): 1104–1117.

Bramoullé, Yann, Rachel Kranton, and Martin D'amours. 2014. "Strategic interaction and networks." *The American Economic Review*, 104(3): 898–930.

Campbell, Benjamin Aaron, Brian M Saxton, and Preeta M Banerjee. 2014. "Resetting the shot clock: The effect of comobility on human capital." *Journal of Management*, 40(2): 531–556.

Chillemi, Ottorino, and Benedetto Gui. 1997. "Team human capital and worker mobility." *Journal of Labor Economics*, 15(4): 567–585.

Crescenzi, Riccardo, Max Nathan, and Andrés Rodríguez-Pose. 2016. "Do inventors talk to strangers? On proximity and collaborative knowledge creation." *Research Policy*, 45(1): 177–194.

De Dreu, Carsten KW. 2006. "When too little or too much hurts: Evidence for a curvilinear relationship between task conflict and innovation in teams." *Journal of management*, 32(1): 83–107.

Depalo, Domenico, Di Addario, and Sabrina Lucia. 2014. "Shedding Light on Inventors' Returns to Patents." *Working Paper*.

Diehl, Michael, and Wolfgang Stroebe. 1987. "Productivity loss in brainstorming groups: Toward the solution of a riddle." *Journal of personality and social psychology*, 53(3): 497.

Dorner, Matthias, Stefan Bender, Dietmar Harhoff, Karin Hoisl, Patrycja Scioch, et al. 2014. "The MPI-IC-IAB-Inventor data 2002 (MIID 2002): Record-linkage of patent register data with labor market biography data of the IAB." *FDZ Methodenreport*, 201406.

Easterly, William, and Ross Levine. 1997. "Africa's growth tragedy: policies and ethnic divisions." *The Quarterly Journal of Economics*, 112(4): 1203–1250.

Fadlon, Itzik, and Torben Heien Nielsen. 2015. "Household responses to severe health shocks and the design of social insurance." National Bureau of Economic Research.

Fapohunda, Tinuke M. 2013. "Towards effective team building in the workplace." *International Journal of Education and Research*, 1(4): 1–12.

Gigone, Daniel, and Reid Hastie. 1997. "Proper analysis of the accuracy of group judgments." *Psychological Bulletin*, 121(1): 149.

Girotra, Karan, Christian Terwiesch, and Karl T Ulrich. 2010. "Idea generation and the quality of the best idea." *Management science*, 56(4): 591–605.

Groysberg, Boris, and Linda-Eling Lee. 2009. "Hiring stars and their colleagues: Exploration and exploitation in professional service firms." *Organization science*, 20(4): 740–758.

Habyarimana, James, Macartan Humphreys, Daniel N Posner, and Jeremy M Weinstein. 2007. "Why does ethnic diversity undermine public goods provision?" *American Political Science Review*, 101(4): 709–725.

Hall, Charles, Dietmar Lindenberger, Reiner Kümmel, Timm Kroeger, and Wolfgang Eichhorn. 2001. "The Need to Reintegrate the Natural Sciences with Economics: Neoclassical economics, the dominant form of economics today, has at least three fundamental flaws from the

perspective of the natural sciences, but it is possible to develop a different, biophysical basis for economics that can serve as a supplement to, or a replacement for, neoclassical economics." *AIBS Bulletin*, 51(8): 663–673.

Hayes, Rachel M, Paul Oyer, and Scott Schaefer. 2005. "Coworker complementarity and the stability of top-management teams." *Journal of Law, Economics, and Organization*, 22(1): 184–212.

Hjort, Jonas. 2014. "Ethnic divisions and production in firms." *The Quarterly Journal of Economics*, 129(4): 1899–1946.

Holmstrom, Bengt. 1982. "Moral hazard in teams." *The Bell Journal of Economics*, 324–340.

Isen, Adam. 2013. "Dying to Know: Are Workers Paid Their Marginal Product?" *Working Paper*.

Jackson, Matthew O, and Asher Wolinsky. 1996. "A strategic model of social and economic networks." *Journal of economic theory*, 71(1): 44–74.

Jaffe, Adam B, and Benjamin F Jones. 2015. *The Changing Frontier: Rethinking Science and Innovation Policy.* University of Chicago Press.

Jäger, Simon. 2016. "How substitutable are workers? evidence from worker deaths." *Working Paper*.

Jehn, Karen A, Gregory B Northcraft, and Margaret A Neale. 1999. "Why differences make a difference: A field study of diversity, conflict and performance in workgroups." *Administrative science quarterly*, 44(4): 741–763.

Jones, Benjamin F. 2009. "The burden of knowledge and the âdeath of the renaissance manâ: Is innovation getting harder?" *The Review of Economic Studies*, 76(1): 283–317.

Jones, Benjamin F. 2010. "Age and great invention." *The Review of Economics and Statistics*, 92(1): 1–14.

Jones, Benjamin F, and Benjamin A Olken. 2005. "Do leaders matter? National leadership and growth since World War II." *The Quarterly Journal of Economics*, 120(3): 835–864.

Jovanovic, Boyan. 1979*a*. "Firm-specific capital and turnover." *Journal of political economy*, 87(6): 1246–1260.

Jovanovic, Boyan. 1979*b*. "Job matching and the theory of turnover." *Journal of political economy*, 87(5, Part 1): 972–990.

La Ferrara, Eliana. 2007. "Descent rules and strategic transfers. Evidence from matrilineal groups in Ghana." *Journal of Development Economics*, 83(2): 280–301.

Lazear, Edward P. 2009. "Firm-specific human capital: A skill-weights approach." *Journal of political economy*, 117(5): 914–940.

Lucas, Robert E, and Benjamin Moll. 2014. "Knowledge growth and the allocation of time." *Journal of Political Economy*, 122(1): 1–51.

Mailath, George J, and Andrew Postlewaite. 1990. "Workers versus firms: Bargaining over a firm's value." *The Review of Economic Studies*, 57(3): 369–380.

Malani, Anup, and Julian Reif. 2015. "Interpreting pre-trends as anticipation: Impact on estimated treatment effects from tort reform." *Journal of Public Economics*, 124: 1–17.

Miguel, Edward, Shanker Satyanath, and Ernest Sergenti. 2004. "Economic shocks and civil conflict: An instrumental variables approach." *Journal of political Economy*, 112(4): 725–753.

Mincer, Jacob A, et al. 1974. "Schooling, Experience, and Earnings." *NBER Books*.

Nguyen, Bang Dang, and Kasper Meisner Nielsen. 2010. "The value of independent directors: Evidence from sudden deaths." *Journal of Financial Economics*, 98(3): 550–567.

Oettl, Alexander. 2012. "Reconceptualizing stars: Scientist helpfulness and peer performance." *Management Science*, 58(6): 1122–1140.

Pentland, Alex. 2012. "The new science of building great teams." *Harvard Business Review*, 90(4): 60–69.

Seaborn.T. 1979. "Talking About the Automat. The Open Channel." Software Patent Institute(IEEE Computer).

Simmel, Georg. 1908. "Sociology: investigations on the forms of sociation." *Duncker & Humblot, Berlin Germany*.

Stein, Jeremy. 2008. "Conversations among Competitors." *American Economic Review*, 98(5): 2150–62.

Stewart, Dennis D, and Garold Stasser. 1995. "Expert role assignment and information sampling during collective recall and decision making." *Journal of personality and social psychology*, 69(4): 619.

Taylor, Alva, and Henrich R Greve. 2006. "Superman or the fantastic four? Knowledge combination and experience in innovative teams." *Academy of Management Journal*, 49(4): 723–740.

Toivanen, Otto, and Lotta Väänänen. 2012. "Returns to inventors." *Review of Economics and Statistics*, 94(4): 1173–1190.

Topel, Robert. 1991. "Specific capital, mobility, and wages: Wages rise with job seniority." *Journal of political Economy*, 99(1): 145–176.

Wageman, Ruth. 1995. "Interdependence and group effectiveness." *Administrative science quarterly*, 145–180.

Waldinger, Fabian. 2010. "Quality matters: The expulsion of professors and the consequences for PhD student outcomes in Nazi Germany." *Journal of Political Economy*, 118(4): 787–831.

Waldinger, Fabian. 2011. "Peer effects in science: Evidence from the dismissal of scientists in Nazi Germany." *The Review of Economic Studies*, 79(2): 838–861.

Wuchty, Stefan, Benjamin F Jones, and Brian Uzzi. 2007. "The increasing dominance of teams in production of knowledge." *Science*, 316(5827): 1036–1039.