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Team-Specific Capital and Innovation†

By Xavier Jaravel, Neviana Petkova, and Alex Bell*

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 percent and −15 percent 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* J24, J31, M54, O31, O34)

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 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

* Jaravel: London School of Economics, Houghton Street, London WC2A 2AE, United Kingdom (email: xavier.jaravel@gmail.com); Petkova: Office of Tax Analysis, US Department of the Treasury, 1500 Pennsylvania Avenue NW, Washington, DC 20220 (email: neviana.petkova@treasury.gov); Bell: Harvard University, 1805 Cambridge Street, Camrbridge, MA 02138 (email: alexanderbell@fas.harvard.edu). This paper was accepted to the *AER* under the guidance of Marianne Bertrand, Coeditor. The opinions expressed in this paper are those of the authors alone and do not necessarily reflect the views of the Internal Revenue Service or the US Treasury Department. We are grateful to Philippe Aghion, Raj Chetty, Ed Glaeser, Larry Katz, Bill Kerr, Josh Lerner, and Andrei Shleifer for advice throughout this project. We thank four anonymous referees for comments that substantially improved the paper. We also thank Daron Acemoglu, Ajay Agrawal, David Autor, Pierre Azoulay, Kirill Borusyak, Gary Chamberlain, Iain Cockburn, Riccardo Crescenzi, Max Eber, Itzik Faldon, Anastassia Fedyk, Andy Garin, Sid George, Duncan Gilchrist, Paul Goldsmith-Pinkham, Ben Golub, Adam Guren, Nathan Hendren, Simon Jaeger, Yuanjian Carla Li, Jonathan Libgober, Matt Marx, Filippo Mezzanotti, Arash Nekoei, Alexandra Roulet, Scott Stern, John Van Reenen, Carl Veller, Fabian Waldinger, Martin Watzinger, Heidi Williams, and seminar participants at the NBER Summer Institute, the NBER Productivity Seminar, and Harvard Labor Lunch for thoughtful discussions and suggestions. Jaravel gratefully acknowledges financial support from the Kauffman foundation. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

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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 life-cycle 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 percent after 8 years), total earnings (-4 percent after 8 years), and citation-weighted patents (-15 percent after 8 years). This evidence implies that the continuation of collaborative relationships has substantial specific value for the typical inventor, approximately equal to one-half of the returns to one year of schooling (Mincer 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 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

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

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 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); and 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 and Di Addario 2014; Aghion and Howitt 1992).

The remainder of the paper is organized as follows. In Section I, we present the dataset and novel descriptive statistics on the composition of teams. In Section II, 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 III, we establish that team-specific capital is a central explanatory channel,

[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 III, we discuss whether other mechanisms could be consistent with the evidence.

ruling out alternative mechanisms that do not operate within teams. In Section IV, we present a series of results delivering insights about the workings of team-specific capital. Section V 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 in Bell 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 five million patents granted from 1976 to today, extracted from the USPTO internal databases in weekly increments.

Administrative data on the universe of US taxpayers are 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 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 and city and state of residence (online Appendix A describes the iterative stages of the match algorithm). The match rate is over 85 percent and the matched and unmatched inventors appear similar on observables, as documented in Bell et al. (2016). Any inventor with a non-US 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 US-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

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

[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

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 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] Using this procedure, 4,714 deceased inventors find an exact match.[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 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 II.

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 (CZ) 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

below 60, i.e., we keep inventors who are 60 in the year of death.

[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 percent 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 online Appendix E, we repeat the analysis with a propensity-score reweighting approach which uses data on all deceased inventors and obtain similar results.

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

deceased inventor. 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 who 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 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 to 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 percent 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 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 percent of the citation distribution.[12] Last, we create indicator variables that turn to 1 when labor earnings

[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 online 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 online 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 percent level and that we obtain similar results when we do not winsorize (see online Appendix Table B14).

[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 II discusses these various robustness checks.

[12] We define the count of patents in the top 5 percent of citations as the number of patents the survivor inventor applied for in a given year that were in the top 5 percent 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

are greater than 0 or above thresholds for the twenty-fifth, fiftieth, and seventy-fifth 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 onward, 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, 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. Online 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 ID, 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 life-cycle 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 group of placebo survivors and real survivors. Importantly, our matching algorithm 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 II that this difference is constant during years prior to co-inventor death, consistent with the assumptions of the difference-in-differences research design. Online Appendix Tables A3 and A4 show that the real and placebo survivors are also similar in terms of the year of

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.

TABLE 1—SUMMARY STATISTICS ON INVENTORS

Variable	Sample	Mean	SD
Panel A. For main analysis			
	Full sample	144,096	316,636
	Real deceased	139,857	308,000
Total earnings	Placebo deceased	139,102	320,970
	Real survivors	177,020	355,347
	Placebo survivors	177,247	360,780
	Full sample	117,559	257,466
	Real deceased	121,691	258,289
Labor earnings	Placebo deceased	124,149	248,546
	Real survivors	152,602	295,832
	Placebo survivors	155,098	290,201
	Full sample	2.31	2.51
	Real deceased	2.50	2.43
Cumulative applications	Placebo deceased	2.50	2.43
	Real survivors	12.42	28.31
	Placebo survivors	11.92	29.52
	Full sample	6.64	12.2
	Real deceased	8.74	13.09
Cumulative citations	Placebo deceased	8.51	13.20
	Real survivors	42.00	171.03
	Placebo survivors	40.20	164.20
	Full sample	43.29	9.65
	Real deceased	50.85	7.44
Age	Placebo deceased	50.85	7.44
	Real survivors	47.53	10.89
	Placebo survivors	47.289	11.16
	Full sample	756,118	
	Real deceased	4,714	
No. inventors	Placebo deceased	4,714	
	Real survivors	14,150	
	Placebo survivors	13,350	

(*Continued*)

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 percent 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 online Appendix Figure A2 present these facts and online Appendix Table A8 indicates that the patterns are similar across technology classes.

As shown on Panel B of online 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 distributions

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

Variable	Sample	Mean	SD
Panel B. For additional analysis			
	Real 2nd-degree connections	175,247	358,347
Total earnings	Placebo 2nd-degree connections	174,900	350,102
	Real coworkers	149,861	312,721
	Placebo coworkers	154,627	316,266
	Real 2nd-degree connections	144,449	291,697
Labor earnings	Placebo 2nd-degree connections	146,674	297,697
	Real coworkers	114,559	258,233
	Placebo coworkers	117,691	256,908
	Real 2nd-degree connections	10.42	42.78
Cumulative applications	Placebo 2nd-degree connections	9.92	25.21
	Real coworkers	2.40	2.58
	Placebo coworkers	2.45	2.52
	Real 2nd-degree connections	37.76	170.11
Cumulative citations	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
	Real 2nd-degree connections	11,264	
No. inventors	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 IB. 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 coworkers, the statistics are computed using data before the year of death. Dollar amounts are reported in 2012 dollars. Online 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 IA and IB.

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 percent. 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

TABLE 2—SUMMARY STATISTICS ON INVENTOR TEAMS

Variable	Sample	Mean	SD	10%	25%	50%	75%	90%
Panel A. Inventor-level statistics on collaborations								
Number of teams per inventor	Full sample	2.58	4.17	1	1	1	3	5
	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 per inventor	Full sample	2.32	3.0	1	1	1	3	5
	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 across teams, for inventors in at least two teams (%)	Full sample	45.32	29.15	16.66	25	33.33	50	100
	Real survivors	31.82	20.84	12.90	18.33	25	37.5	50
	Placebo survivors	32.13	21.10	13.22	18.75	26.08	37.5	50

	Team size	Mean	p10	p25	p50	p75	p90
Panel B. Team-level statistics for real and placebo survivors, by team size							
Number of distinct commuting zones across co-inventors	2	1.212	1	1	1	2	2
	3	1.766	1	1	2	2	3
	4	1.795	1	1	2	2	3
	5	2.057	1	1	2	3	3
	6	2.312	1	1	2	3	4
Team heterogeneity (coefficient of variation for total earnings, within team)	2	0.391	0.051	0.150	0.330	0.609	0.945
	3	0.434	0.065	0.158	0.346	0.612	0.999
	4	0.414	0.084	0.199	0.372	0.611	0.950
	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 IB. 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 online Appendix Table A9 for additional evidence on geographic dispersion and online Appendix Tables A10, A11, A12, A13, and online Appendix Figure A3 for additional evidence on within-team heterogeneity. For a detailed description of the data sources and sample construction, see Sections IA and IB.

at least 25 percent 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 reported in online Appendix Tables A10, A11, and A12. For teams of two inventors, we study the extent of assortative matching nonparametrically using the absolute difference between outcomes for each of the co-inventors. The results, reported in online 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

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

statistics, in Section IV we investigate the question of which team structures are most conducive to the accumulation of team-specific capital.

Online 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 percent 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] Online 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 IV.

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 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 percent. When conditioning on patents that were assigned to different assignees,[16] the percentage falls but remains high, at 21.6 percent. 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 IV. 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.

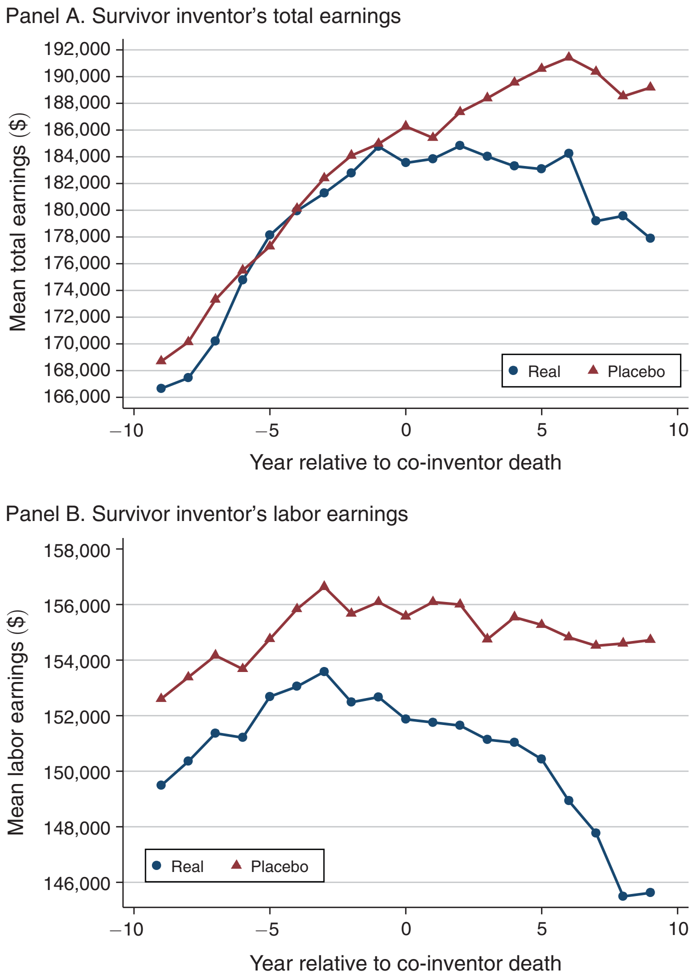
A. *Research Design*

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

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

[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.



(*Continued*)

Figure 1. Path of Outcomes around Co-Inventor Death

survivor inventors are on a different earnings and patent trajectory than the full population of inventors. To address this challenge, we use the control group of placebo survivor inventors described in Section I 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 nonparametrically 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

[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, see Malani 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.

Panel C. Survivor inventor's adjusted forward citations received for patents applied in year

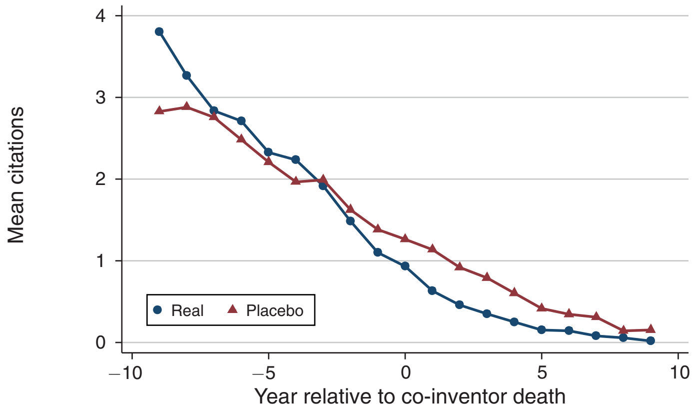


Figure 1. Path of Outcomes around Co-Inventor Death (*Continued*)

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 nine-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 nine years. The unbalanced nature of this panel is the same for real and placebo inventors. Online Appendix Figure B2 shows that the results are similar on a balanced sample. Dollar amounts are reported in 2012 dollars. Refer to Section IB for more details on the sample and to Section IC for more details on the outcome variables.

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 percent 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.

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]

[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 online Appendix

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.[21] If the identification assumption described 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[22] 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:[23]

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

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

Table B12, we use the coupled bootstrap procedure of Abadie and Spiess (2015) to estimate standard errors, taking into account the matching step.

[21] 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 imprecisely estimated. Results are qualitatively similar when all observations are kept.

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

[23] We exclude observations with inventors below the age of 25 or above the age of 70 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 quasi-maximum likelihood estimate 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 QMLE 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 Gourieroux et al. 1984). Also note that these specifications, whether with ordinary least squares 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. Online Appendix E offers an in-depth discussion of these issues.

estimator[24] to our setting addresses the concern that age, year, and individual fixed effects may not fully account for trends in lifetime earnings and patents around co-inventor death. An 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 $E[\mathbf{1}_{\{L_{it}^{All}=k\}}\epsilon_{it} \mid L_{it}^{Real}, L_{it}^{All}, a_{it}, t, i] = 0 \ \forall (t,k)$, then $\beta^{Real}(k)$ gives the causal effect of co-inventor death on the outcome of interest k years after death. Online 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 1 after the time of co-inventor death for real survivor inventors ($AfterDeath_{it}^{Real}$) and another dummy turning to 1 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.[25] This specification is as follows:

$$Y_{it} = \beta^{Real} AfterDeath_{it}^{Real} + \beta^{All} AfterDeath_{it}^{All} + \sum_{j=25}^{70} \lambda_j \mathbf{1}_{\{age_{it}=j\}} + \sum_{m=1999}^{2012} \gamma_m \mathbf{1}_{\{t=m\}}\, \alpha_i + \epsilon_{it}. \tag{2}$$

C. *Results*

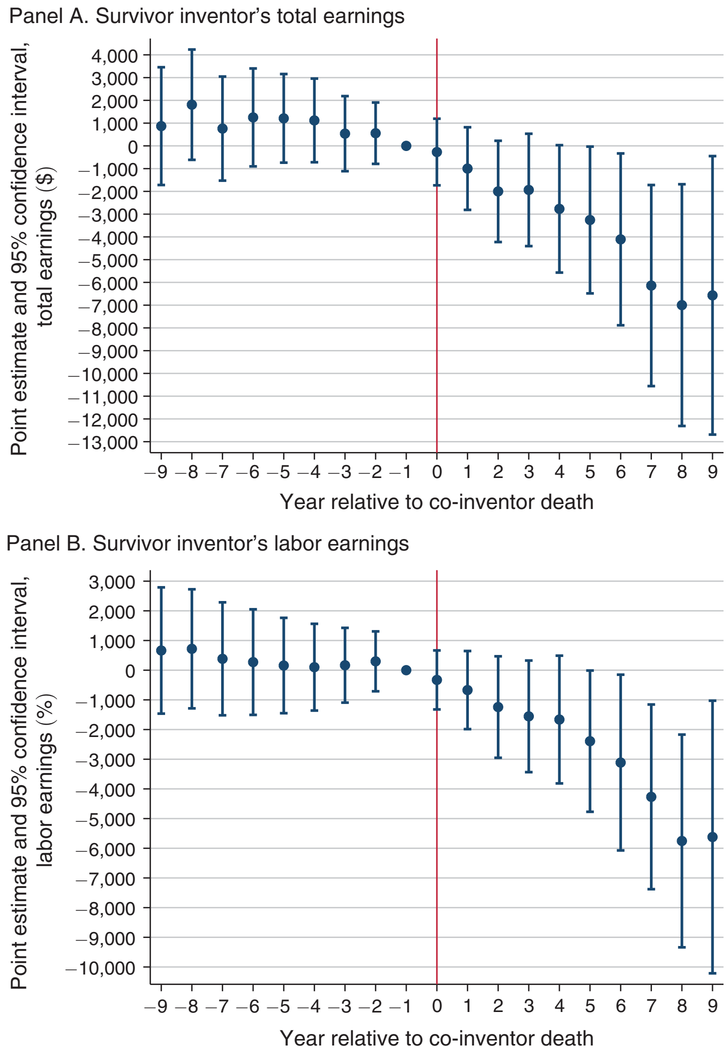
Figure 2 reports the point estimates and 95 percent confidence interval for the coefficients β_k^{Real}, obtained from specification (1). Four outcome variables are considered: total earnings, labor earnings, non-labor earnings, and citations. The

[24] 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* × *Post* dummy turning to 1 after treatment for the treated, and a *Post* dummy common to both the treated and control groups. In our setting, where co-inventors' deaths 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* × *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).

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



(*Continued*)

FIGURE 2. DYNAMIC CAUSAL EFFECTS OF CO-INVENTOR DEATH

point estimate on the lag turning to 1 in the year preceding death is normalized to 0 and inference is carried out relative to this lag.[26] 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

[26] The full set of leads and lags L_{it}^{Real} always sum up to 1 for the survivor inventors and our specification includes individual fixed effects, therefore one of the leads and lags must be "normalized" to 1. Online Appendix D discusses this standard normalization more formally.

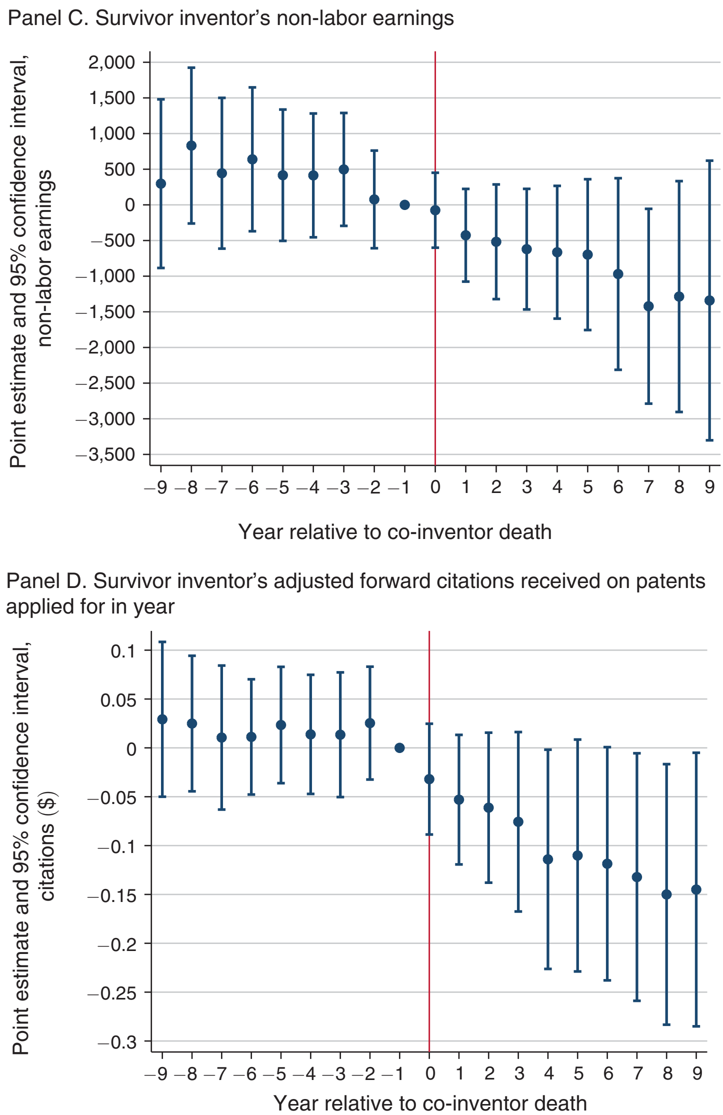


Figure 2. Dynamic Causal Effects of Co-Inventor Death (*Continued*)

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 IIB, 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 percent level. Dollar amounts are reported in 2012 dollars. The sample includes all real and placebo survivor inventors in a nine-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 nine years. The unbalanced nature of this panel is the same for real and placebo inventors. Online Appendix Table B6 shows that the results are similar on a balanced panel. For more details on the outcome variables, refer to Section IC.

outcomes, rather than a level shift, and that co-inventor death has effects beyond short-term disruption of teamwork. As further discussed in Section III, 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-inventor death, the real survivor inventors' total earnings are \$7,000 lower (4 percent of mean total earnings in the sample of survivors), their labor earnings are about \$5,800 lower (3.8 percent of mean labor earnings in the sample of survivors), and their citation-weighted patent production is 15 percent lower than it would have been had they not experienced the premature death of a co-inventor.[27] About 80 percent of the total decline in earnings is due to a decline in labor earnings.

In order to reduce noise, we use specification (2), with a single indicator turning to 1 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 twenty-fifth, fiftieth, and seventy-fifth 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 Figure 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 III. 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.[28]

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 online Appendix Table B6 and are similar to the results using the unbalanced panel.

Long-Term Persistence.—The finding that co-inventor death has a long-lasting effect is a striking result of this paper. Online Appendix Table B3 confirms that the effect becomes larger over time in a statistically significant way, using a specification

[27] 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 an 8 percent 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 percent 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 percent in physics and 16.5 percent in chemistry.

[28] 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}):

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

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

	Total earnings	>p25	>p50	>p75	Non-labor earnings
Panel A. Survivor inventor's total earnings and non-labor earnings					
$AfterDeath^{Real}$	−3,873	−0.01531	−0.0107	−0.00772	−1,199
Standard error	(910)	(0.00434)	(0.00457)	(0.0039)	(498)
$AfterDeath^{All}$	−223	0.00036	0.00066	−0.00068	651
Standard error	(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
Number of survivors	27,500	27,500	27,500	27,500	27,500
Number of deceased	9,428	9,428	9,428	9,428	9,428
Estimator	OLS	OLS	OLS	OLS	OLS
	Labor earnings	>0	>p25	>p50	>p75
Panel B. Survivor inventor's labor earnings					
$AfterDeath^{Real}$	−2,715	−0.00913	−0.01039	−0.007203	−0.00638
Standard error	(706)	(0.00315)	(0.00411)	(0.0037)	(0.00342)
$AfterDeath^{All}$	−38	−0.0051	−0.00259	−0.00066	0.00127
Standard error	(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
Number of survivors	27,500	27,500	27,500	27,500	27,500
Number of deceased	9,428	9,428	9,428	9,428	9,428
Estimator	OLS	OLS	OLS	OLS	OLS

	Patent count	Citation count	Count of patents with no citations	Count of patents in top 5 percent of citations
Panel C. Survivor inventor's patent applications and forward citations				
$AfterDeath^{Real}$	−0.09121	−0.09024	−0.07656	−0.02182
Standard error	(0.02063)	(0.02326)	(0.0217)	(0.00789)
$AfterDeath^{All}$	0.00055	0.04084	0.00325	0.00455
Standard error	(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
Number of survivors	27,500	27,500	27,500	27,500
Number of 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 1 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. Online 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: (i) patent count is the number of patents the survivor inventor applied for in a given year; (ii) 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); (iii) 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; (iv) the count of patents in the top 5 percent of citations is the number of patents the survivor inventor applied for in a given year that were in the top 5 percent 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 nine-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 nine years. The unbalanced nature of this panel is the same for real and placebo inventors. Online Appendix Table B6 shows that the results are similar on a balanced panel. Standard errors are clustered around the deceased inventors.

with an indicator turning to 1 for observations more than four years after death (which reduces the noise reflected by the standard errors shown in 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.[29] To address this issue, online 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 online Appendix Table B4 and are qualitatively similar to the findings reported in Figure 2. In Section IV, 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 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.[30] We show the relevance of this channel by documenting in online Appendix Tables B1 and B2 that the effect is gradual primarily in technology categories with a slow "speed of patenting."[31] 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 IV) but much less gradual than in technology categories where it takes a longer time. In

[29] 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.

[30] 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) for Finland; Depalo and Di Addario et al. (2014) for Italy; and Bell et al. (2016) for the United States.

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

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 online Appendix B robustness checks using alternative proxies.

Additional Robustness Checks.—Online Appendix B reports a series of additional robustness checks showing that the results do not stem from lingering health conditions (online Appendix Figure B3 and Table B7), are similar with a propensity-score reweighting strategy (online Appendix Figure B4 and Table B8), are robust to considering alternative measures of citations (online Appendix Tables B9 and B10), and are robust across technology classes (online Appendix Table B11). We also show that the results preserve strong statistical significance with an inference procedure taking into account the matching step (online Appendix Table B12) and are similar when using log transformations and non-winsorized variables (online 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 II.

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.[32]

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.[33] Online Appendix Figure C1 shows similar

[32] 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 I for more details about the definition of these groups and the construction of the sample.

[33] 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.

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.

Online 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 II do not result from the disruption of the firm or the R&D lab following an inventor's death.[34] 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.[35] We have checked that similar 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 online 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 percent 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 II.[36] 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

[34] 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 (online Appendix Table C17) and that the magnitude of the effect is not correlated with firm size (online Appendix Table C22).

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

[36] 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.

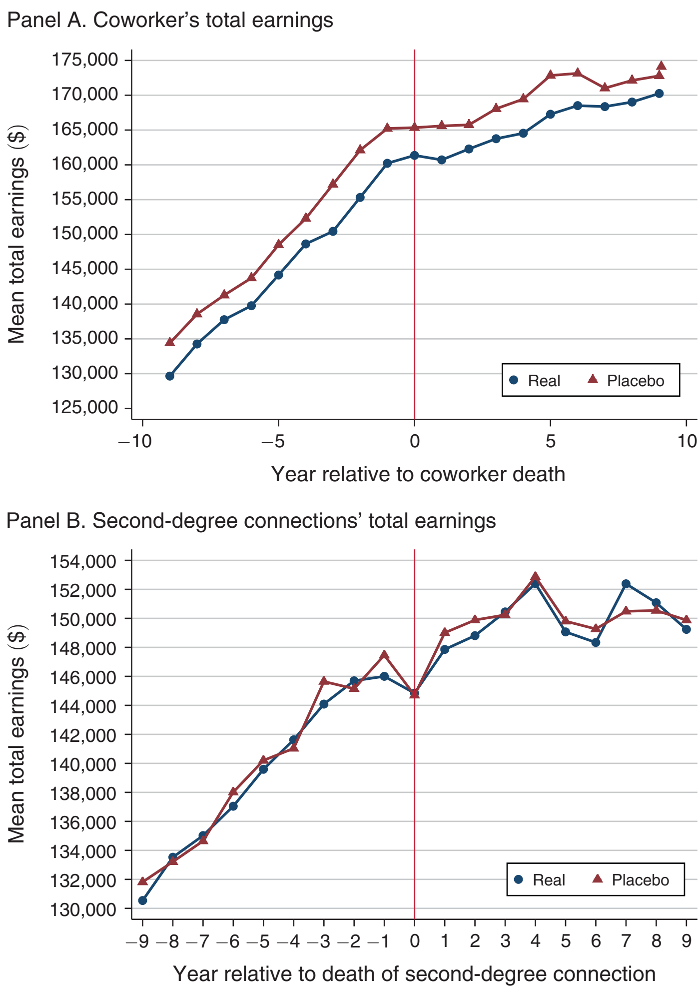


Figure 3. Path of Outcomes for Coworkers and Second-Degree Connections around Death

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 nine-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 nine 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 IB for more details on the sample and to Section IC for more details on the outcome variables.

a firm are substitutable while there is no strong complementarity or substitutability patterns between inventors who are two nodes away in the co-invention network.[37]

[37] 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 online Appendix C for a complete discussion.

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 online 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 online 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 (online 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 (online Appendix Table C17).[38]

Ruling Out Other Mechanisms.—We examine other mechanisms in which team-specific capital plays no role in online Appendix C, including the loss of "person-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 I, 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 II. 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 online 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 afterward. The use of our control group of placebo survivor inventors is sufficient to

[38] 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. Online Appendix Table C14 documents heterogeneity in the treatment effect for teams in academia versus the private sector.

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 II 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. Online Appendix Table C5 shows that the distribution of earnings changes is very similar for the real and placebo survivor inventors.[39]

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

Moreover, online 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 online 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 online Appendix Table C8 shows these

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

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
Standard error	(1,553)	(1,328)	(1,680)	(1,081)
2	−3,573	−2,798	−810	−1,308
Standard error	(2,111)	(1,178)	(1,675)	(1,278)
3	−5,656	−4,151	−3,243	−2,939
Standard error	(2,612)	(1,968)	(1,632)	(2,562)
4	−6,566	−5,132	−4,853	−7,037
Standard error	(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 16 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 online Appendix Table C4. Under the identification assumption described in Section IIB, 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.

results.[40] Moreover, panel B of online Appendix Table C8 shows that the effect is much larger when the deceased was a "star," in the top 2 percent 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 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.

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

[40] 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.

Table 5—Collaboration Patterns between Deceased and Survivor Inventors before Death

Variable	Sample	Mean	SD	10%	25%	50%	75%	90%
No. patents	Real	8.114	17.285	1	1	3	9	18
	Placebo	7.41082	12.757	1	1	3	8	18
No. 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
No. real survivors	14,150							
No. placebo survivors	13,350							

Notes: The variables are defined as follows: (i) # patents is the number of patents of the survivor before co-inventor death; (ii) # co-patents is the number of patents co-invented by the survivor and the deceased before co-inventor death; (iii) co-patent share is the share of the survivor's patents that were co-invented with the deceased before death; (iv) collaboration length is the number of years that elapsed between the first and last joint patent application between the survivor and the deceased; (v) 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.

(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:

$$(3)\qquad Y_{it} = \beta^{Real} AfterDeath_{it}^{Real} + \eta^{Real} X_i \cdot AfterDeath_{it}^{Real} + \beta^{All} AfterDeath_{it}^{All} + \eta^{All} X_i \cdot AfterDeath_{it}^{All} + \sum_{j=25}^{70} \lambda_j \mathbf{1}_{\{age_{it}=j\}} + \sum_{m=1999}^{2012} \gamma_m \mathbf{1}_{\{t=m\}} + \alpha_i + \epsilon_{it},$$

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.[41]

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 co-patents explains 75 percent of the average effect on total earnings,

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

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
Standard error	(22.552)	(17.164)	(8.342)	(0.00085)	(0.00069)
Collaboration length	−1,063.253	−523.296	−323.296	−0.0245	−0.02892
Standard error	(405.382)	(228.55)	(118.516)	(0.01072)	(0.01537)
Collaboration recency	447.921	360.281	110.728	0.00508	0.00482
Standard error	(145.592)	(139.825)	(50.95)	(0.00256)	(0.00266)
Number of co-patents	42.163	64.029	20.231	0.0015	0.00127
Standard error	(107.372)	(121.255)	(431.156)	(0.01962)	(0.0124)
Number of patents	−49.129	5.022	−60.001	−0.00642	−0.00442
Standard error	(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
Standard error	(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
Number of survivors	27,500	27,500	27,500	27,500	27,500
Number of 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.

78 percent of the average effect on labor earnings, 70 percent of the average effect on patent count, and 54 percent of the average effect on citation count. Similarly, a one standard deviation increase in collaboration length explains 47 percent of the average effect on total earnings, 33 percent of the average effect on labor earnings, 46 percent of the average effect on patents, and 53 percent of the average effect on citations. Lastly, a one standard deviation increase in collaboration recency explains 45 percent of the average effect on total earnings, 52 percent of the average effect of labor earnings, 22 percent of the average effect on patents, and 21 percent of the average effect on citations. This indicates that the effect is driven by the loss of a co-inventor with whom the survivor was collaborating extensively.[42]

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 on the patents of the survivor, but now we only consider patents that were not co-invented with the deceased.[43]

[42] 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.

[43] 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 not list the name of the deceased were

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
Standard error	(0.01525)	(0.01815)	(0.01525)	(0.00478)
AfterDeath^{All}	0.1162	0.08578	0.05763	0.0247
Standard error	(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
Number of survivors	27,500	27,500	27,500	27,500
Number of 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: (i) patent count is the number of patents the survivor inventor applied for in a given year, excluding all patents co-invented with the deceased; (ii) 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; (iii) 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; (iv) the count of patents in the top 5 percent of citations is the number of patents the survivor inventor applied for in a given year that were in the top 5 percent of the citation distribution, excluding all patents co-invented with the deceased. The sample includes all real and placebo survivor inventors in a nine-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 nine years. The unbalanced nature of this panel is the same for real and placebo inventors. Standard errors are clustered around the deceased inventors.

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 percent) relative to the effect on the total number of patents of the survivor documented in Table 3 (around −9 percent). 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.[44]

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. Online Appendix Table C9 shows that the effect increases by about 10 percent for all of our outcomes when the degree of co-invention overlap increases by one standard

indeed invented without the active collaboration of the deceased.

[44] 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 II: innovation is a stochastic process and the placebo survivors gradually outperform the real survivors.

deviation. This finding suggests that there are negative feedback effects when more collaborators in a given inventor's network 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.[45] Next, we turn to a closer investigation of the sources of team-specific capital.[46]

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 II and III 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.[47] First, where does team-specific capital come from? We distinguish 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 literature offers 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,

[45] We spoke with 14 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.

[46] Online 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. Online 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.

[47] 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.

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 literature on contract theory and the sociology of teams suggests 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 respective effort levels.[48] The insight of Holmström (1982) is that moral hazard in teams can be solved by the introduction of group incentives.[49] 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.[50] We find empirical support for the moral hazard channel in Section IVB.

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.[51] Our setting allows us to examine a related question: does within-team heterogeneity make a team harder to replace? We 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.

[48] 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).

[49] 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.

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

[51] See, e.g., Easterly and Levine (1997); Alesina and Spolaore (1997); La Ferrara (2007); Miguel, Satyanath, and Sergenti (2004); Habyarimana et al. (2007); and 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.

B. *Match versus Experience Components of Team-Specific Capital*

In this section, we carry out tests of the "match" and "experience" views described in Section IVA. 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.[52]

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.[53] Lazear (2009, p. 932) 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 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.[54] Intuitively, under the match view the newly-hired team member immediately becomes a perfect replacement for the inventor who 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 (CZ), 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. Online

[52] Note that the slow dissipation of rents from previous collaboration, documented in Section II, 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 rematch. 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.

[53] 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.

[54] 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 rematching 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 rematching 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.

Appendix 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 rematch: survivors are more likely to find new co-inventors if they work in an environment with more inventors similar to them. Online Appendix Tables D2 and D3 show similar results using alternative proxies for local inventor labor market thickness.[55] Online Appendix Table D4 shows that our proxy for market thickness becomes predictive of the speed of rematch 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 "fixed match quality."[56] 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.[57]

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.[58] 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).[59] 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

[55] 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.

[56] 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.

[57] 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.

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

[59] Online 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.

TABLE 8—MATCH AND EXPERIENCE COMPONENTS OF TEAM-SPECIFIC CAPITAL

	Total earnings	Labor earnings	Non-labor earnings	Patents	Citations	New co-inventor
Panel A. Heterogeneity by number of inventors in survivor's NBER technology subcategory within CZ-EIN						
$AfterDeath^{Real}$ × *InventorNumber* (*SD*)	50.237	−76.711	90.821	0.00912	−0.00512	0.228
Standard error	(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
Number of survivors	25,089	25,089	25,089	25,089	25,089	25,089
Number of deceased	8,554	8,554	8,554	8,554	8,554	8,554
Estimator	OLS	OLS	OLS	Poisson	Poisson	OLS

	Total earnings	Labor earnings	Non-labor earnings	Patent count	Citation count
Panel B. Heterogeneity by length of potential collaboration					
$AfterDeath^{Real}$ × *Potential Collaboration Length*	−983.345	−619.342	−254.462	−0.0246	−0.0214
Standard error	(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
Number of survivors	27,500	27,500	27,500	27,500	27,500
Number of deceased	9,428	9,428	9,428	9,428	9,428
Estimator	OLS	OLS	OLS	Poisson	Poisson

(*continued*)

the survivor's life cycle, thus addressing the possible correlation with match quality: we do so in online Appendix Tables D5 and D6 and obtain similar results.[60]

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 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 rematching.[61]

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

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

[61] 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 II that the gradual nature of the effect can be explained by the fact that innovation is a stochastic and long-term process.

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

	Total earnings	Labor earnings	Non-labor earnings	Patent count	Citation count
Panel C. Are the returns to experience quadratic?					
$AfterDeath^{Real}$ × *Potential Collaboration Length*	−901.523	−640.212	−280.462	−0.0216	−0.0223
Standard error	(346.538)	(266.754)	(146.838)	(0.00981)	(0.01062)
$AfterDeath^{Real}$ × (*Potential Collaboration Length*)^2	38.534	−50.211	60.231	0.00145	−0.002012
Standard error	(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
Number of survivors	27,500	27,500	27,500	27,500	27,500
Number of 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					
$AfterDeath^{Real}$ × *Potential Collaboration Length* × *Age at First Collaboration*/10	583.345	380.342	154.462	0.00882	0.00924
Standard error	(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
Number of survivors	27,500	27,500	27,500	27,500	27,500
Number of 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 specification (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.

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 IVA.

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 measures of within-team heterogeneity from Section ID 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 percent for the various outcomes. Controls interacted with treatment status ensure that these results are not driven by top-down spillovers or life-cycle effects.[62]

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 who 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's (1908) hypothesis by identifying triads that were 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 Table 9 shows the results: the magnitude of the treatment effect is about 15 to 30 percent 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 online Appendix Tables D8 and D9, ensuring that these results are not driven by top-down spillovers or life-cycle effects. These results suggest that team-specific capital operates through social dynamics and increased trust between inventors.

In online 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 (online Appendix Table D10) and we find no significant heterogeneity by team size (online Appendix Table D11).

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

Table 9—Heterogeneity by Team Structure

	Total earnings	Labor earnings	Non-labor earnings	Patent count	Citation count
Panel A. Heterogeneity by degree of within-team heterogeneity					
$AfterDeath^{Real} \cdot \frac{\overline{CV}}{SD(\overline{CV})}$	−522.912	−421.242	−126.120	−0.01892	−0.01710
Standard error	(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
Standard error	(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
Number of survivors	27,500	27,500	27,500	27,500	27,500
Number of deceased	9,428	9,428	9,428	9,428	9,428
Estimator	OLS	OLS	OLS	Poisson	Poisson
Panel B. Heterogeneity by triadic closure					
$AfterDeath^{Real} \cdot DeceasedClosedTriad$	−813.313	−787.35	−179.85	−0.01843	−0.021431
Standard error	(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
Standard error	(1,543.21)	(1,070.214)	(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
Number of survivors	1,360	1,360	1,360	1,360	1,360
Number of 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 forward citations, and labor earnings, standardized by its standard deviation). Panel B uses the sample of "closed triads," defined in the main text in Section IVC, 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 life-cycle 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 percent after 8 years), total earnings (−4 percent after 8 years), and citation-weighted patents (−15 percent 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.

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