Selection and Firm Survival

Evidence from the Shipbuilding Industry, 1825-1914

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Several alternative theories of firm performance can explain the well-known observation that survival is negatively related to age, but all of them exploit the idea that age serves as a proxy for an omitted variable. However, a more mundane explanation – selection bias driven by variations in firm quality – may also underlie the phenomenon. This paper employs a 90-year plant-level panel data set on the US iron and steel shipbuilding industry of the 19th and early 20th centuries to discriminate between the two explanations. The shipbuilding industry exhibits the usual joint dependency of survival on age and size, but this dependency is eliminated after controlling for heterogeneity by using pre-entry experience as a proxy for firm quality. The evidence points to a dominant role of selection bias in the age-dependency of survival. At the same time, pre-entry experience is found to have a large and extremely persistent effect on survival, and this finding is inconsistent with standard explanations for the role of pre-entry experience on firm performance.

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1. Introduction

One of the most prominent empirical regularities to emerge in industrial organization is the age dependency of firm survival. The relationship has been observed in large multi-industry samples constructed from manufacturing censuses (Dunne, Roberts and Samuelson [1988, 1989 - US]; Disney, Haskel and Heden [2000 – UK]; Baldwin *et al.* [2000 – Canada]; Persson [2002 – Sweden]), Dun and Bradstreet data (Evans [1987a, 1987b]), and Compustat data (Hall [1987]), as well as in numerous specialized samples (Audretsch [1991], Audretsch and Mahmood [1995], Baldwin and Gorecki [1991], Mata and Portugal [1994], and Wagner [1994]). Importantly, because age and size are positively correlated among surviving firms, and size and survival are positively correlated, the age-dependency of exit is robust to controlling for firm size.[1]

The relationship appears to be an attractive target for modelers. Because age-dependency is an intriguing and challenging regularity to explain, we have come to expect that models able to do so will be revealing about the way industries evolve. A number of competing explanations now exist, but they all exploit the idea that age serves as a proxy for an omitted variable. First, firms may accumulate knowledge over time, through learning by doing or as the outcome of an active research program. Knowledge reduces the exit hazard but it is unobservable to the econometrician. The usual proxies for knowledge – cumulative output for learning by doing and “stocks” of past R&D expenditures or patents for active research – are imperfectly correlated with accumulated knowledge, so age appears to reduce directly the exit hazard whether or not such proxies are included in the regression.

There are also some recent models, without learning, in which a variable correlated with age is unobserved by the econometrician. Cooley and Quadrini [2001] construct a model in which financial market frictions induce variations in the

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[1] And thus models in which size is a sufficient statistic for exit are inconsistent with the evidence.

debt-equity ratio that are correlated with age, that affect the survival rate, and that have not to date been included in hazard regressions.[2] Most recently, Klepper and Thompson [2002] construct a model in which industries are composed of distinct, but essentially unobservable, submarkets. Firm survival is increasing in the number of submarkets in which a firm is active and this number is in turn correlated with age.

However, there could be a more mundane explanation for the age-dependency of survival. It is well-known that unobserved heterogeneity in the exit hazard induces the appearance of age effects on survival. As a cohort of firms ages, the risk set becomes increasingly composed of firms with the lowest propensity to exit. The mean exit rate for the cohort therefore declines with cohort age, even if the exit hazard does not decline with age for any individual firm. A formal demonstration is straightforward. Let $\lambda(\theta)$ be the constant conditional hazard of exit for type θ firms, $F(\theta,t)$ the associated distribution of survival durations, and $G(\theta)$ the initial distribution of types of a given cohort. Then the unconditional hazard at age t is

$$\lambda(t) = \int \lambda(\theta)\left[1 - F(\theta,t)\right] dG(\theta)\,.$$

Differentiating with respect to time, we have

$$\frac{d\lambda(t)}{dt} = -\int \lambda(\theta)\frac{dF(\theta,t)}{dt}\,dG(\theta) \leq 0\,.$$

If age-dependency can be explained by unobserved heterogeneity, the regularity would turn out to be rather unrevealing. After all, no one expects all firms to be equally capable, and discovering that they are not teaches us little.

We would therefore like to be able to discriminate between these competing classes of explanations. This paper presents the results of a particularly simple test. Using a new data set on the iron and steel merchant shipbuilding industry,

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[2] Clementi and Hopenhayn [2002] have shown that such financial market frictions are efficient in the presence of moral hazard.

which is described in detail in Section 2, I construct proxies for firm quality that predict the propensity to exit. I show first that the familiar size-conditional age-dependency of exit is present in the data. I then ask whether the addition of these proxies to the hazard regression eliminates the age-dependency of the hazard. The answer is that it does: after conditioning on firm type the hazard is independent of age. The results therefore imply that the initial age-dependency can be explained by selection bias.[3]

This paper uses data on firms' pre-entry experience as a proxy for firm quality. The results clearly show that the effects of pre-entry experience do not decay even over a very long horizon, and they are not diminished by controlling for size. We have long known that firms and entrepreneurs with prior experience

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[3] An explanation for the lack of reference to Jovanovic's [1982] selection model is in order. Conventional wisdom is that Jovanovic's model was the first to predict a positive relationship between age and survival, but this interpretation is incorrect. Dunne, Roberts, and Samuelson [1989] seem to have introduced this notion to the profession. Recall the basic mechanism of selection and learning in Jovanovic. Firms do not initially know their quality, but learn it over time through observation of noisy signals. Firms that come to believe with sufficient precision that their quality is below an exit threshold, choose to exit. Dunne Robert and Samuelson report that "failure rates should a decreasing function of plant age" because older plants have precise estimates of quality, making subsequent revisions of their estimates large enough to cross the threshold less likely. They correctly observe that the result requires that the quality threshold does not rise too rapidly with age, and proceed to assume that is the case. However, for almost any reasonable parameterization, this assumption cannot generally hold. Firms will not exit until they have had a few signals, so the hazard must initially be rising for all firms. The hazard for the lowest quality firms may rise monotonically with firm age, while the hazard for the highest quality firms, which must eventually fall to zero, cannot vary monotonically with age. Moreover, the notion that more signals increase precision is not general. Jovanovic assumes, for convenience, a normal conjugate for the distribution of beliefs about quality. It should be recalled that this is the *only* conjugate family for which more signals unambiguously implies greater precision (Chamley [2003, ch. 2]). Pakes and Ericson [1998] produce a counterexample with a Beta conjugate distribution in which the exit hazard is *cyclical* with respect to age. Jovanovic's model does predict the correct relationship between age and mean growth, but this is driven by a process of learning, not selection (which is obvious once one recalls that the relationship holds whether or not some firms exit). Pakes and Ericson [1998] accurately describe their analysis of the growth implications of Jovanovic's model as a test of the implications of passive learning rather than selection..

closely related to their new venture survive much longer than those without relevant experience,[4] and there is now emerging a body of evidence that the pre-entry experience of a firm or its founders has extremely persistent effects (Carroll *et al.* [1996], Geroski, Mata, and Portugal [2002], Klepper [2002a, 2002b, 2003], Klepper and Simons [2000]).[5] These regularities by themselves may reflect nothing more than learning spillovers or hysteresis effects.[6] However, the new finding in this paper that pre-entry experience effects show no tendency to diminish over long periods of time even after controlling for size is inconsistent with the standard rationalizations for the role of pre-entry experience. Theories that can explain this remarkable effect of pre-entry experience may well be revealing about the way industries evolve.

2. The Data

The simple test undertaken in this paper involves some formidable data challenges. First, we need to track firm entry and exit over a sufficiently long period

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[4] Lane [1989; ATM machines], Mitchell [1991; diagnostic imaging], Carroll *et al.* [1996; autos], and Klepper and Simons [2000; televisions] show that diversifying firms with experience in related fields perform better along a variety of dimensions than less experienced entrants. Dunne, Roberts and Samuelson [1988] find that diversifying firms survive longer and grow faster than *de novo* entrants. Sleeper [1998; lasers], Klepper and Sleeper [2001; lasers] and Walsh, Kirchhoff and Boylan [1996; semiconductor silicon] report that spin-offs survive longer than other startups, and Klepper [2002b; autos] further shows that the quality of a spin-off's parent matters. Eisenhardt and Schoonhoven [1990; semiconductors] report that firm performance is increasing in the industry experience of their founding teams. Helfat and Lieberman [2001] have a helpful review of the literature.

[5] This findings echoes Jovanovic and Rousseau's [2001] observation that something about the firms that went public during the 1920s (which the authors attribute quite generically to firm quality and label "organization capital") has served them well enough to account for greater stock market capitalization than real cumulative investment would have predicted, even to the present day.

[6] For example, pre-entry experience may help firms secure financing and allow them to expand more rapidly (Burton, Sørensen and Beckman [2001]), or it may induce permanent differences in post-entry performance when initial performance differences are locked in by dynamic scale economies (Klepper [2002a:41]).

of time to produce estimates of the effects of age that can be distinguished from the effects of calendar time. Second, we need accurate firm-level output data over this extended period to control for firm size. Third, these data must be supplemented with meaningful proxies for quality. Few researchers have had access to data on output and survival that combines information on pre-entry experience with data on post-entry performance prior to exit.[7]

This paper meets the challenge with new data that allow me to assess the effects of measurable heterogeneity induced by variations in prior experience on the age-survival relationship in the U.S. iron and steel merchant shipbuilding industry. The data consist of a record of all metal vessels built in the United States and subject to documentation requirements from the inception of the iron and steel shipbuilding industry in 1825 until preparations for war began to transform the industry after 1914.[8] Plant-level panel data covering this 90-year period are supplemented by company histories of many of the firms that built the vessels. This section provides a brief description of data sources, coverage and limitations. Extensive discussion of the data, especially of coding decisions made in the translation from textual firm histories to sample data, can be found in a companion paper (Thompson [2002]).

The study tracks the rise of metal shipbuilding from its early, experimental days to eventual dominance over wooden shipbuilding. Shipbuilding is one of the oldest industries in America.[9] By 1825, when the first iron-hulled vessel -- a minnow of 14 tons named *Codorus* -- was launched in 1825 in York, PA, the output of wooden vessels exceeded 100,000 tons per year. The *Codorus* undertook a

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[7] King and Tucci [2002] and Geroski, Mata and Portugal [2002] are two exceptions.

[8] The data sources allow me to continue the sample to 1919, but the explosion of government cost-plus contracts and government yard-construction subsidies makes the period after 1914 less interesting for the present study. During this period, some firms even made a profitable business charging the government for ocean-going vessels made out of concrete (Haviland [1962]).

[9] Hutchins [1948] provides a detailed chronological history of the industry. Brown [1951} is an excellent source for the earliest years.

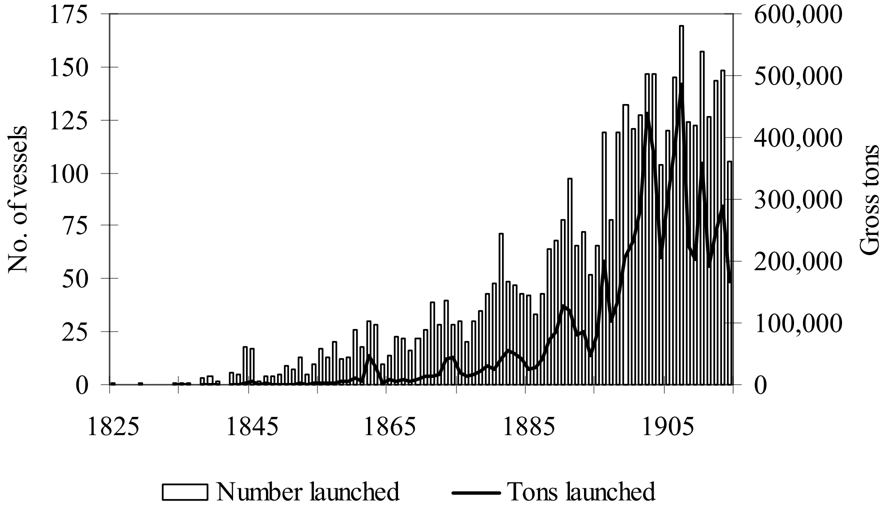

FIGURE 1. Iron and steel vessels launched by private yards, 1825-1914.

widely-reported journey up the Susquehanna River but did little to stimulate additional experiments with the new hull material, and the development of the industry was remarkably slow. In fact, iron shipbuilding did not amount to much at all until the last quarter of the 19th century. In the 50 years to 1874, only 61 firms entered the industry. Of these, 48 firms had abandoned production by 1875, only two of them having launched more than ten vessels. A mere 560 vessels had been launched and, with a total capacity of 301,000 gross tons, iron vessels accounted for less than three percent of total shipbuilding output. Average vessel size, at just 536 tons, had also remained modest, and only two vessels exceeding 5,000 tons had been launched. These numbers stand in stark contrast to their counterparts for the forty-year period 1875-1914, during which time 212 producers entered, and 3,550 vessels with an average capacity of 1,729 tons were launched for a total of over 6.14 million tons, including 346 ships exceeding 5,000 tons (see Figures 1 and 2). By the end of this period, metal had fully supplanted wood as the material of choice, and vessels were being built of a size far in excess of anything ever launched by wood shipbuilders.

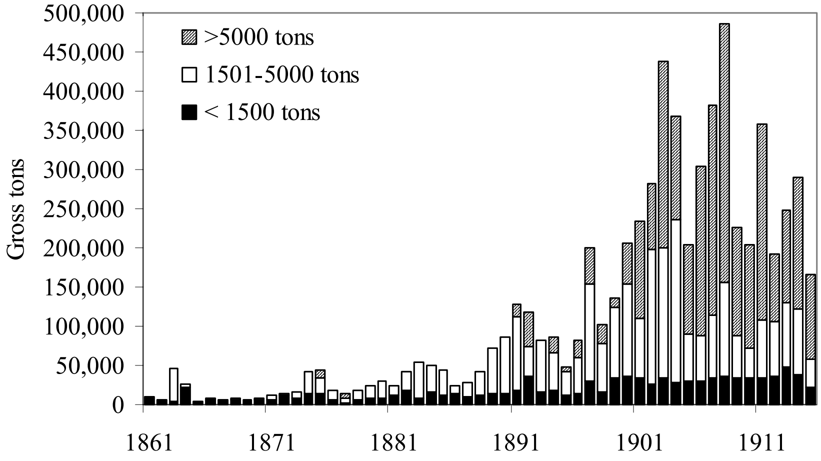

FIGURE 2. Total deliveries of metal-hulled vessels, by vessel size (gross tons).

Largely because of its capital intensity, the metal shipbuilding industry differed from wood shipbuilding in fundamental ways. Wood ship and boat building had for most of its history been a widely dispersed cottage industry. The tools required to produce a wooden vessel, even one in excess of 1,000 tons, amounted to no more than a $500 investment. Skilled carpenters could turn out a boat in their own backyards if they had access to water. If they relocated, the boatyard was as easily moved as the family furniture. Building in iron or steel, in contrast, was a major industrial enterprise requiring specialized and site-specific machinery, a network of industrial suppliers, and access to substantial capital.[10] Once established, an iron boat- or shipyard and its equipment was essentially immobile. If local conditions proved unfavorable, the yard went bankrupt.

These distinctions between metal and wooden shipbuilding justify their treatment as distinct industries. Data availability further motivate the singular atten-

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[10] Cramp [1902:2] points out that the Philadelphia yard of William Cramp & Sons owned a single derrick, the value of which was "considerably greater than that of William Cramp's entire ship-yard sixty years ago. . . which was not then surpassed by any other ship-yard on the Delaware."

tion on the metal shipbuilding industry. Since 1789, US law has required that all vessels exceeding 20 gross tons capacity be registered with the government. The registration documents contain basic technical details about each vessel but have always been of limited value to researchers because they are scattered, incomplete, and, above all, they do not record the builder. This study takes as a starting point a rather fortunate find in the National Archives (Bureau of Navigation [c.1920]). Around 1920, William Lytle, an employee of the Bureau of Navigation with a penchant for making lists, constructed a record of metal vessels built in the United States since 1825. The register, a hand-written leather-bound volume, is based on official documents but, remarkably in view of the work it must have entailed, the register also lists the builder for most of the vessels. The register is not quite a finished product. A large number of early vessels were omitted entirely, others were not assigned to builders, and others were incorrectly assigned. Corrections were made using diverse sources, especially Brown [1951] and typescript vessel lists held in various specialized manuscript collections. A more extensive omission is that the register reports only merchant vessels. Vessels built by private companies for the U.S. Customs Service, the U.S. Coast Guard, and the U.S. Navy were added from official records provided in Bauer and Roberts [1991], Benham and Hall [1913], Canney [1993, 1995, 1998], Conway [1979], Still [1996], and US Coast Guard [1989].

The sample analyzed in this paper was restricted to producers who launched at least one vessel in excess of the 20 gross tons capacity required to trigger registration.[11] However, in order to track more precisely the dates of activity of the included firms, the sample includes all metal vessels known to have been produced by the firms, regardless of gross tonnage. The restricted sample contains technological details of exactly 4,000 vessels constructed by 273 producers. Figure

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[11] Not all of the vessels in the database were registered, and some of those that were registered were not required to be. Fifty-seven builders, accounting for 114 boats, were excluded by the minimum size criterion.

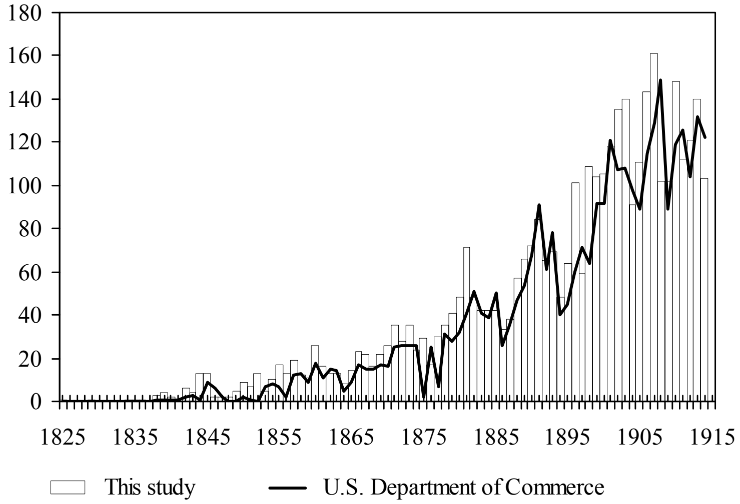

Figure 3. Metal Vessels Launched for Civilian Use, 1825-1914. Department of Commerce records allocate production vessels to year of registration or enrollment, whereas the data set allocates vessels to year of launching. These dates coincide in the majority of cases. Source for USDC data: Smith and Brown [1948, Table 6].

3 compares the number of vessels for which I have information with the US Department of Commerce's (USDC) official tally of metal vessels built during the period. To facilitate comparison with the USDC tally, the figure excludes military vessels in the sample, all vessels under 20 gross tons capacity, and all vessels for which no gross tonnage is available, leaving a count of 3,482 vessels. The USDC tally reports 3,222 documented merchant vessels, missing 260 merchant vessels that should have been included. I believe that, subject to the minimum size requirement, the database is the most complete record in existence of metal shipbuilding in the United States prior to 1915.

The only major omission that I am aware of is a failure to systematically include vessels constructed for export, and which therefore were not documented in the United States. Few firms exported vessels and, for most of those that did, export activity formed a minor part of their total production. There are a couple of exceptions, however. Between 1878 and 1914, James Rees and Sons of Pittsburgh, PA, produced hundreds of knock-down iron and steel steamboats for service on South American rivers (Rees and Sons [1913]), but only fourteen of their vessels are recorded in the United States. Marine Iron Works of Chicago, IL, also sold an unknown number of knock-down iron vessels to South America, although in their case the majority of their export trade consisted of the sale of machinery along with plans for wooden hulls to be built locally (Marine Iron Works [1902]).

To supplement the panel data on vessels, detailed textual histories have been produced for as many of the 273 recorded producers as possible. For some small producers, I have no information at all, while for others I have identified their pre-entry backgrounds only from city directories indicating their profession prior to entry. For most large producers, in contrast, extremely rich histories are available to document in some detail the way in which they entered and left the industry. The median producer lies between these extremes, and brief textual histories were constructed from a variety of sources, including manuscript records, county histories, obituaries, genealogical records, and (on several occasions) from information kindly provided by descendents of the shipbuilders.[12]

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[12] Some difficulties arise in constructing the count of firms, and assigning vessels to them. First, the source data contain many vessels assigned to individuals who were in fact employed by another firm. Addressing this problem required substantial but straightforward detective work. The individual to whom a vessel had been attributed often proved to be the superintendent of construction, an owner of the firm, or even an owner of the vessel. Often, it was possible to find biographies of the individuals which placed them in a firm at the right time. In other cases, a cross-check with vessel lists for large firms linked the individual to the firm. Sometimes, company records provided employee lists, and in one case genealogical records linked several individuals to a common business. A second difficulty is that it is not always a straightforward matter to decide when a reorganization or relocation constituted the creation of a new producer or the continuation of the old one. It was necessary to make frequent judgments about the boundaries between firms. The underlying criterion was to code as a new firm any organization where there was reason to believe the operation of the firm had been substantively affected by a reorganization, or where the technological capabilities of the plant were believed to be substantively different. To illustrate the use of this

Coding of the pre-entry backgrounds of shipbuilders was concerned primarily with distinguishing firms or founders of firms that entered after gaining experience in manufacturing iron- and steel-hulled vessels, wood-hulled vessels or engines from those that entered with different backgrounds. Entrants with prior experience of vessel construction were experienced in hull design and marketing vessels, and had often earned a solid reputation for quality and reliability among vessel buyers. Firms with pre-entry experience in engine manufacturing were likely to be particularly technically competent, despite lacking direct experience in hull construction. In fact many shipbuilders, particularly builders of wooden vessels, were not equipped to manufacture engines and, although many operated small foundries to manufacture custom iron fittings, the normal practice was to sub-contract the major machinery to specialists. These indicators of relevant experience are compared with three other categories: firms whose pre-entry experience consisted of foundry work; firms with diverse pre-entry experiences including shipping, dredging, construction, railroads, rail car manufacturing, naval engineering, and iron milling; and firms whose pre-entry experience is not known.[13]

Table 1 provides some summary statistics for the sample. Entrants that had previously been involved in shipbuilding or engine manufacturing are combined into a single variable, EXPERIENCED, and these firms are compared with others that had neither of these backgrounds. Of the 273 firms in the sample, 122 belong to the EXPERIENCED group, 48 had previously operated a FOUNDRY without having ventured into engine manufacturing, 50 are classified with prior experience in a range of MISCELLANEOUS fields, and 70 have UNKNOWN pre-entry backgrounds. The mean entry year for each group of firms is within five years of the

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criterion in practice, some examples are provided in the appendix.

[13] Firms often had pre-entry experience in various fields, and when this experience was significant, multiple coding was made. In most cases, however, only a single, dominant, experience was coded.

overall sample mean, and the ranges and standard deviations are similar across groups. That is, there is no clear pattern over time in the types of firms entering.

Table 1

Entry Year, Life, and Entry Size, by Prior Experience

		SOURCE OF PRIOR EXPERIENCE			
	TOTAL	EXPERIENCED[a]	FOUNDRY[b]	MISCELLENOUS[c]	UNKNOWN
Number of Firms	273	122	48	50	70
Entry Year:					
MEAN	1889	1884	1886	1889	1894
MIN	1825	1825	1842	1844	1835
MAX	1914	1914	1914	1914	1914
STD. DEV	20.4	21.7	20.1	20.4	17.3
Duration (years):					
MEAN	10.2	15.4	6.1	12.2	2.4
MIN	0.5	0.5	0.5	0.5	0.5
MAX	110.5	92.5	57.5	110.5	21.5
STD. DEV	18.3	19.9	10.8	23.5	4.4
First-year Production:					
VESSEL SIZE (RATIO)[d]	0.56	0.79	0.35	0.66	0.19
VESSELS BUILT (NO.)	1.48	1.70	1.44	1.56	1.10
TONS DELIVERED	1,273	1,920	496	990	257

I use the first year in which a metal vessel was launched to mark a firm's entry, and the last year a vessel was launched, plus 0.5 (to avoid simultaneous entry and exit), to denote its time of exit. For firms with an unknown exit date after 1914, I code 1914 as the censoring year. [a] Shipbuilding or engine manufacturing. [b] Firms in this class include some also listed under Miscellaneous. [c] Includes shipping, dredging, construction, railroads, rail car manufacturing, naval engineering, and iron milling. [d] Average vessel size (gross tonnage) in a firm's first year expressed as a ratio of the average size of all vessels launched in the same year.

There are, however, significant differences across categories in survival and characteristics at entry. Experienced firms survived on average 15.4 years, longer than any other group. In contrast, foundry owners, the group with the least amount of recorded relevant experience, survived on average only 6.1 years. The diverse group of firms classified with miscellaneous backgrounds, comprising experiences in shipping, construction, and railroads among others, predictably fall between the two previous groups, with an average life of 12.2 years.

Table 1 also shows that in the first year after entry experienced firms launched more, and larger, vessels than other types of entrants. Moreover, although their earliest vessels were smaller than the average size of vessels launched by incumbents, the ratio of entrant to incumbent vessel size was clearly influenced by experience. In fact, experienced firms began producing vessels more than twice as large as FOUNDRY operators, and launched 20 percent more of them. Thus, experienced firms entered the industry on a larger scale than inexperienced firms.

Inevitably, firms for which I have been able to code backgrounds are much more likely to be among the successful, the significant, or the strange; they produced more, and larger, vessels, and survived an average five times as long as the average uncoded firm. In the absence of further information, I take the crude approach of creating a category UNKNOWN, and run analyses both with this category and after excluding all firms with unknown backgrounds. However, the differences between coded and uncoded firms are sufficiently large that the bias towards the more successful firms in the textual histories raises the possibility that any measured effects of experience are biased upwards by the missing data. Fortunately, pre-entry experience is the only variable missing for these firms, and it is feasible to use Monte Carlo simulations to evaluate the potential effects of missing data. As I shall show later, the key results handily survive the simulation exercise.

3. Results

The results are reported in four sections. If pre-entry experience serves as a proxy for firm quality, and if it to be capable of inducing spurious conditional age effects on survival, there must be a large and lasting effect of experience. Section A explores whether pre-entry experience meets these demands. Using a parametric hazard model that does not condition on quality, Section B shows that the data exhibit the usual joint effects of size and age on survival. Section C then shows that the introduction of the quality proxies eliminates the large and significant effect of age previously found. Finally, Section D reports results from the Monte Carlo simulations.

A. The magnitude and persistence of prior experience effects

Table 2 reports the results of four Cox proportional hazard regressions, with right-censoring of observations at 2001.[14] The first two columns exclude the 70 firms with unknown pre-entry backgrounds, while the last two columns include them. The baseline hazard is calculated for firms with foundry experience only, and the coefficient estimates reflect that their performance differs substantially from other experience groups. All four regressions include controls for location and year of entry, the coefficients of which (not reported) do not suggest any noteworthy patterns.[15]

The key result is the estimated hazard ratio for firms with pre-entry experience in shipbuilding or engine manufacturing.[16] Column (1) returns a ratio for

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[14] Although output data are available only through 1914, firm survival is observed to the present day.

[15] The location dummies allocate production to four regions: the Atlantic and Gulf coasts, the Pacific coast, the Great lakes, and the Western rivers. Five time dummies distinguish entrants before 1861 and in each decade thereafter.

[16] As one would expect, the hazard ratio for firms with unknown backgrounds, when these are in-

Table 2

Hazard Ratios from Cox Proportional Hazards Models

Unknown backgrounds:	excluded		included	
Censoring Year:	2001	1945	2001	1945
Experienced	0.467^{**} (.10)	0.539^{***} (.12)	0.442^{***} (.09)	0.501^{***} (.11)
Miscellaneous	0.865 (.21)	0.913 (.22)	0.823 (.20)	0.865 (.21)
Unknown	----	----	1.755^{**} (.41)	1.882^{***} (.44)
Experienced x Age	1.042^{**} (.02)	1.003 (.02)	1.043^{***} (.02)	1.006 (.02)
Miscellaneous x Age	1.003 (.02)	0.989 (.02)	1.004 (.02)	0.990 (.02)
Unknown x Age	----	----	1.008 (.05)	0.985 (.05)
Test of Proportionality [a]	$\chi^2_{12}=3.9$	$\chi^2_{12}=7.1$	$\chi^2_{14}=4.2$	$\chi^2_{14}=7.4$
Number of Yards	203	203	273	273
Number of Failures	173	167	236	230

Standard errors in parentheses. Significance levels: *=10%, **=5%, ***=1%. Estimated with Efron's approximation to the partial likelihood function. Not reported are the hazard ratios for three location dummies and five time dummies. [a] Tests on the proportionality assumption, due to Gramsbh and Therneau [1994], are conducted on the scaled Schoenfeld residuals as approximated by Stata. Following the suggestions of Hosmer and Lemesho [1999: 205-7], a logarithmic transformation of the time scale is used.

Experience of 0.47. Entry with relevant experience reduces the hazard rate upon entry by about one half relative to the baseline. However, the significant coefficient of 1.042 on the Experience x Age interaction indicates that the ef-

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cluded in the sample, is large. The ratio for the diverse firms categorized into Miscellaneous does not differ significantly from one.

fect of pre-entry experience is not persistent. Each year of post-entry experience raises the hazard ratio for experienced firms by 4.2 percent. Thus, after ten years the hazard ratio for EXPERIENCE has risen to 0.69, while the effect of pre-entry experience is eliminated entirely after eighteen years of post-entry experience. These results are replicated in column (3), which includes the 70 firms whose backgrounds are unknown.

It turns out that the apparent decay of experience effects indicated in columns (1) and (3) is due entirely to a modest number of firms that survived through World War II, some of which continued operations for many years after.[17] Columns (2) and (4) repeat the analysis after censoring all observations at 1945. Doing so eliminates entirely the slow decay of the experience effect. As columns (2) and (4) indicate, the hazard ratio for experienced firms is still about one half that of inexperienced firms, but there is now no evidence that the effect decays with time.

It is certainly no surprise that the enormous changes experienced by the industry during the war years should undermine the effects of experience obtained at least 35 years previously. But until the war, experienced firms have a persistently lower hazard than inexperienced firms. Although the sample contains only firms that entered by 1914, there is no evidence that the effects of experience decline before the onset of World War II.[18]

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[17] The firms are (dates of operation in parentheses): American Shipbuilding's Lorain yard (1899−1985), American Shipbuilding's Buffalo yard (1900−1962), Dubuque Boat and Boiler Works (1905−1972), Charles Seabury and Co. (1893−1955), Pusey and Jones (1854−1946), Bath Iron Works (1905−) Great Lakes Towing Co. (1907−), and Newport News (1891−).

[18] Lane [1951], the official history of the wartime shipbuilding program, provides extensive details of the unique conditions brought about by the demands of war production. In particular, yards with significant structures and equipment paid for by the government were able to secure ownership of this capital on extremely favorable terms after the war.

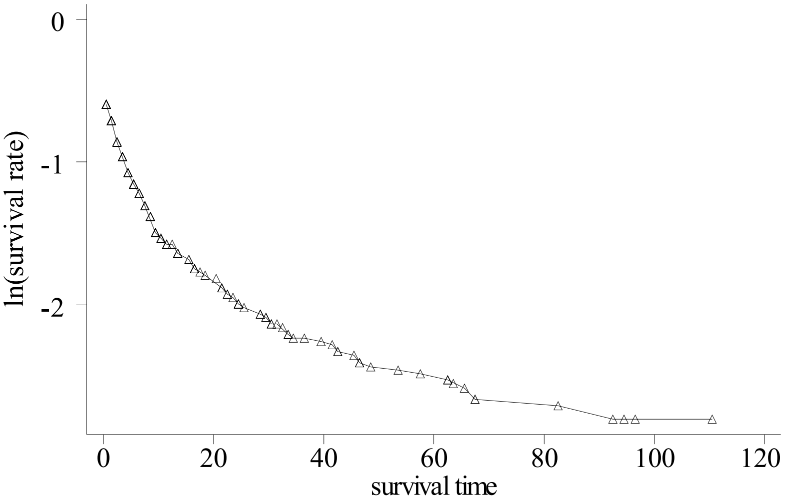

FIGURE 4. Log survival rates. Baseline hazards from column (1) of Table 2. The symbols indicate the baseline hazard corresponding to each observed failure time. The curve is a cubic spline fitted through these points. The baseline corresponds to a firm entering in 1859 on the Atlantic coast with prior experience in foundry work.

B. Size, age and survival without conditioning on quality

It is useful to explore the joint effects of size and age within the framework of a fully parametric model. The logarithm of the baseline survival curve from the proportional hazards model of Table 2, column (1), is plotted in Figure 4. The curve is convex, consistent with a hazard declining monotonically with respect to firm age. Thus, a Weibull hazard model may provide an acceptable parametric representation. The remainder of the analysis is therefore based on hazard functions taking the form

$$h_i(t, x_{it}, \beta, \lambda) = (\lambda + 1)t^{\lambda} e^{x_{it} \ln \beta},$$

where t is the age of firm i, x_{it} is a vector of covariates, some of which are timevarying, and β is a vector of hazard ratios. The single parameter, λ, of the Weibull hazard function has a useful interpretation as an elasticity: it measures the percentage change in the hazard induced by a one-percent increment to age.

The first column of Table 3 includes current output, measured by the number of vessels launched each year, as the only covariate. Size is negatively related to the exit hazard. Each one unit increment to output reduces the exit hazard by about 7.5 percent so that a yard launching, for example, five vessels is about 33 percent more likely to survive the next year than one launching a single vessel. At the same time, the elasticity of the hazard with respect to firm age is a highly significant –0.25, indicating that increases in firm age induce a marked reduction in the hazard. That is, the shipbuilding industry exhibits the familiar dependence of survival on size *and* age.

The strong effect of age reported in column (1) may exaggerate its true role if size is poorly measured. Figure 5, which plots annual numbers of vessels launched for four of the most successful producers in Philadelphia, illustrates that yards produced vessels in small numbers and the number of contracts won could vary markedly from year to year. Consequently, output in the current calendar year may be a rather poor measure of effective size. Column (2) attempts to smooth out these large variations in annual output rates with an alternative measure of firm activity, denoted SCALE, which measures for each year the average annual rate of output since entry. On the basis of the log likelihood, this variable explains rather more than current output. Moreover, although current output and SCALE have the same units of measurement, the hazard ratio on SCALE is much smaller: a firm with an average annual rate of output of, say, three instead of two vessels, faces a 30 percent lower hazard. As expected, the age effect is reduced, but it remains economically important and statistically significant.

Table 3

Joint Size and Age Effects on Survival

(Hazard Ratios from Weibull Hazard Models)

	OUTPUT AS NUMBER OF VESSELS			OUTPUT AS GROSS TONNAGE (‘000S)
	(1)	(2)	(3)	(4)
CURRENT OUTPUT	0.924** (.03)	––––	––––	––––
SCALE OF PLANT	––––	0.698*** (.06)	0.674*** (.06)	0.944** (.02)
CURRENT MARKET SHARE	––––	––––	1.019*** (.00)	1.008 (.01)
INDUSTRY OUTPUT	––––	––––	1.000 (.00)	1.000 (.00)
λ	–0.254*** (.03)	–0.126** (.04)	–0.094** (.04)	–0.201*** (.04)
LN L	–453.7	–443.4	–439.0	–442.8

Standard errors in parentheses. Significance levels: *=10%, **=5%, ***=1%. All regressions include the location and time indicators given in footnote 16.

Finally, columns (3) and (4) introduce further controls for size. In column (3), market share and industry output are added. In column (4), each output measure is replaced with data on gross tonnage launched rather than numbers of vessels launched. In both cases the (size-conditional) age-dependency of firm survival remains strong and significant.

C. Effects of quality on the relationship between age, size and survival

It has been shown so far that (i) pre-entry experience has large and persistent effects on survival, and therefore meets the minimal demands for a meaningful proxy for firm quality; (ii) the industry exhibits the familiar size-conditional age-dependency of survival that has appeared in so many studies. This section

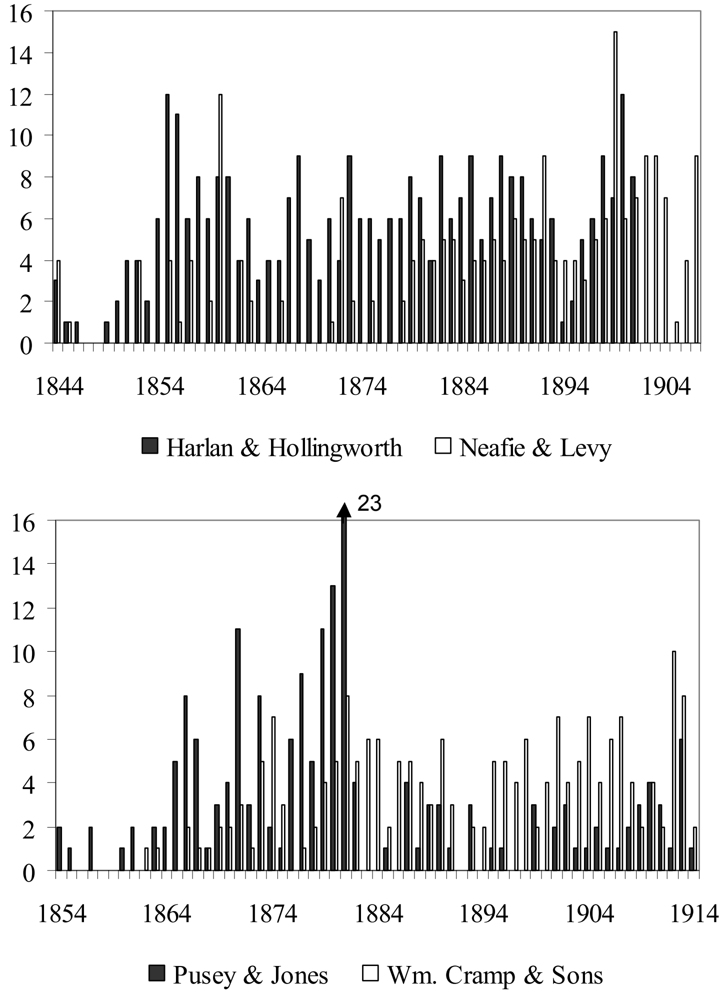

FIGURE 5. Vessels launched by four successful Philadelphia yards. The unusual output of Pusey & Jones in 1881 is accounted for by a single contract for 12 small sailing scows – some iron, some steel – which could be constructed two to a berth.

addresses two questions:

- Is the first result robust to the inclusion of controls for firm size (thereby eliminating the possibility that experience effects are due to hysteresis)?
- Is the second result robust to the inclusion of proxies for quality?

The results are reported in Tables 4 and 5. Column (1) of Table 4 combines the three size variables from column (3) of Table 3 with the experience indicator variables. On the first question, note that the point estimates for the hazard ratios on EXPERIENCED and MISCELLANEOUS are essentially unchanged from those obtained under the Cox proportional hazards specification, even though the regression now has several controls for size. Conditional on age, size, location, and entry period, the exit hazard of firms with prior experience in shipbuilding or engine manufacturing is about half the hazard for firms with prior experience in foundry work. On the second question, note that the elasticity of the hazard with respect to age has declined by half to a statistically insignificant –0.05. That is, about fifty percent of the hazard reduction initially attributed to age is explained by the fixed measures of heterogeneity. Selection bias induced by the early failure of low-quality firms is clearly important, while models based on unobserved variables correlated with age do not seem to be. Moreover, to the extent that unobserved heterogeneity continues to exist within experience groups, this estimate of the dominant role of selection bias must be a lower bound.

The remaining columns in Table 4, and Table 5, report further regressions designed to test the robustness of these results. In column (2) of Table 4, experience is decomposed into its component parts of shipbuilding and engine building. Both are found to have similar effects on the hazard. Column (3) removes from the sample firms with unknown backgrounds. The measured effect of experience is

Table 4

Hazard Ratios from Weibull Hazard Models

			Unknown Backgrounds Excluded	Entry Post-1875
	(1)	(2)	(3)	(4)
Scale of plant	0.734***	0.725***	0.779***	0.746**
	(.06)	(.06)	(.06)	(.09)
Current Market Share	1.018***	1.018***	1.017***	1.060
	(.00)	(.00)	(.01)	(.05)
Industry Output	1.000	1.000	1.000	0.997
	(.00)	(.00)	(.00)	(.00)
Experienced	0.531***	– – – –	0.581**	0.507***
	(.12)		(.13)	(.13)
. . . . In Shipbuilding	– – – –	0.623**	– – – –	– – – –
		(.14)		
. . . . In Engine Building	– – – –	0.538***	– – – –	– – – –
		(.13)		
Miscellaneous	0.760	0.783	0.839	0.769
	(.20)	(.20)	(.22)	(.24)
Unknown	1.687**	1.729**	– – – –	1.683**
	(.42)	(.43)		(.42)
λ	–0.047	–0.043	–0.088*	–0.006
	(.04)	(.04)	(.05)	(.04)

Standard errors in parentheses. Significance levels: *=10%, **=5%, ***=1%. All regressions include the location and time indicators. Log-likelihoods are not comparable across columns and are not reported.

Table 5

More Hazard Ratios from Weibull Hazard Models

	(1)	(2)	(3)	(4)
			Entry Post- 1875	
		Unknown backgrounds excluded		
Scale of plant	0.589^{***}	0.654^{***}	0.541^{***}	0.429^{***}
	(.08)	(.08)	(.09)	(.09)
Current Market Share	1.019^{***}	1.018^{***}	0.978	1.053
	(.00)	(.01)	(.05)	(.05)
Industry Output	0.998	0.999	0.990^{**}	0.993^{**}
	(.00)	(.00)	(.01)	(.00)
Cumulative output	1.011^{***}	1.009^{***}	1.042^{***}	1.050^{***}
	(.00)	(.00)	(.01)	(.01)
Experienced (Age <10)	0.597^{**}	0.639^{*}	0.557^{**}	0.544^{**}
	(.13)	(.15)	(.16)	(.14)
Experienced (Age 10- 20)	0.399^{***}	0.462^{**}	0.444^{**}	0.389^{**}
	(.14)	(.16)	(.18)	(.15)
Experienced (Age >20)	0.378^{***}	0.443^{**}	0.442	0.334^{*}
	(.13)	(.17)	(.28)	(.21)
Miscellaneous	0.697	0.787	0.759	0.677
	(.19)	(.21)	(.26)	(.23)
Unknown	1.644^{**}	– – –	– – –	1.641^{**}
	(.42)			(.46)
λ	–.008	–0.048	–0.058	0.010
	(.05)	(.05)	(.06)	(.06)

Standard errors in parentheses. Significance levels: *=10%, **=5%, ***=1%. All regressions include the location and time indicators. Log-likelihoods are not comparable across columns and are not reported.

not altered by the change in sample, although the estimate of λ has increased sufficiently that it is significant at the 10 percent level. Because experience may be more important in the early, experimental days of the industry, column (4) restricts the sample to firms entering after 1875. Experience continues to matter as strongly as before, and age continues to have no impact on the hazard.

Table 5 reports additional regressions that allow for three piecewise constant effects for experience at different ages. First, consistent with the Cox regressions of Table 3, there is no evidence that the effects of experience decay with firm age. In fact, the effect of pre-entry experience on the exit hazard is even greater for firms over 20 years of age than it is for firms under 10 years of age, and these results are obtained despite the inclusion of several controls for size. Second, this alternative treatment of prior experience strengthens the result that age no longer influences survival. These results are unchanged regardless of whether one includes or excludes firms with unknown prior experience, or whether one restricts the sample to post-1875 entrants.

D. Monte Carlo simulation of missing data.

The 70 firms with unknown backgrounds performed worse than any other group. It is likely that the majority of these firms were inexperienced, not only because they preformed poorly, but also because their absence from the hundreds of shipbuilding and county histories, city almanacs, and biographical compendiums consulted during data collection suggests that they had not previously been active in any related business. Nonetheless, it is also likely that some of these firms were experienced, and have just fallen through the cracks. If a sufficient number of them were experienced, the missing data will have led us to exaggerate the effect of pre-entry experience.

In this section, I report the results of a simulation exercise that assesses the extent to which the missing data could account for the reported results. The simulation imagines what could be considered a worst case scenario, namely that

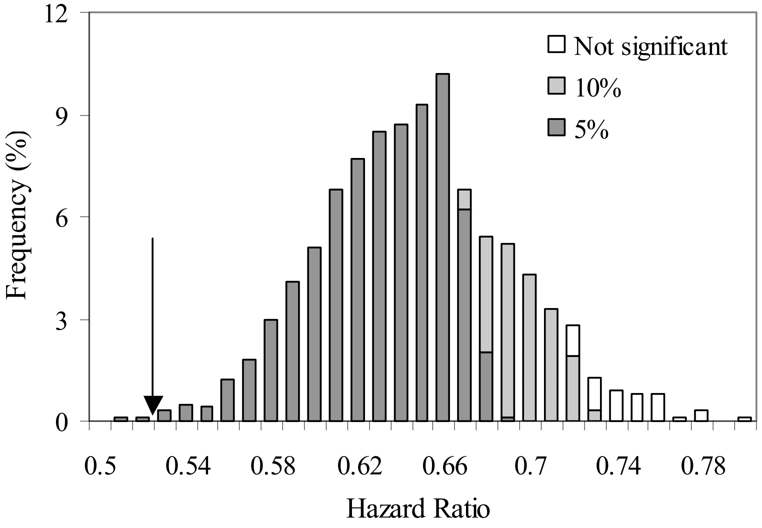

FIGURE 6. Estimated hazard ratio for experience from random assignment of firms with unknown backgrounds to experienced and inexperienced firms, 1,000 replications. Regression specification is identical to column (1) of Table 4. Arrow indicates point estimate from column (1) of Table 4.

the firms with missing data were just as likely to be experienced as the firms for which data are available. To implement this, each of the 70 firms with unknown backgrounds was assigned at random to either the foundry, experienced, or miscellaneous groups with probabilities equal to the proportion of each of these three groups found in the data. The hazard regression from column (1) of Table 4 is then run with these artificial assignments of firms.

Figure 6 plots the estimated hazard ratios for EXPERIENCE obtainedfrom 1,000 replications of this exercise. The mean hazard ratio is 0.65, compared with 0.53 in Table 4, and only one estimate in the 1,000 runs exceeds 0.80. Figure 6 also reports the significance levels: 76 percent of the estimates are significant at the five percent level or greater, and fully 95 percent of the estimates are significant at the ten percent level or greater. By any reasonable standard[19] the simulation results clearly show that the effect on survival of pre-entry experience is not driven by missing data.

4. Conclusions

A number of complex theories of firm performance can explain the well-known observation that survival is negatively related to age, but all of them exploit the idea that age serves as a proxy for an omitted variable. However, a more mundane explanation – selection bias driven by variations in firm quality – may also underlie the phenomenon. Using new data for the US iron and steel shipbuilding industry, this paper has presented the results of a simple test to discriminate between these two classes of explanations for the age-dependency of survival. It was found that the age-dependency observed in the data can be explained by selection bias, leaving nothing for the more complex theories to explain.

Assuming these results apply more generally[20], they suggest that complex theories to explain the size-conditional effects of age on firm survival and growth[21] may well be barking up the wrong tree. Instead, simple models of firms

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[19] For example, the simulations pass Sala-i-Martin's [1997] concept of robustness with flying colors.

[20] The challenging data requirements of the test demand narrow industry studies. The shipbuilding industry was chosen because it provided a unique opportunity to combine an unusually long panel of plant-level data with textual firm histories. However, some features of the shipbuilding industry, notably the absence of strong dynamic or static economies of scale, distinguish it from some of the more frequently studied episodes in US manufacturing history (e.g. the automobile industry), and more work needs to be done to replicate the findings of this paper in other industries. But the early signs are that what distinguishes shipbuilding from industries where scale economies might be more important will not be a factor in the results. In the US automobile industry, where dynamic scale economies appear to be central to understanding the industry's evolution, Klepper [2003] has found that controlling for a firm's background can all but eliminate the effect of age even without controls for size.

[21] The empirical regularity appears to be that growth is declining in size and age when samples are conditioned on survival, and on size only without conditioning. Thus, it may be the apparent age-dependency of survival that creates the age-dependency of growth (Klepper and Thompson [2002]).

performance may well be consistent with the evidence on age-dependency. For example, in Hopenhyan's [1992] model of exit driven by exogenous productivity shocks, age has no effect on the exit hazard after conditioning on size. However, the simple addition of some fixed firm effects – either in the variance of the productivity shocks, or the sensitivity of firm profitability to the shocks – are sufficient to make the model consistent with the evidence presented in this paper.

This paper used the pre-entry backgrounds of firms as a proxy for firm quality. It was found that relevant pre-entry experience has large effects on survival, that the effects showed no tendency to diminish as firms gained post-entry experience, and that they were not diminished by controlling for firm size. The observed persistence of experience effects is inconsistent with traditional rationalizations of why experienced firms may outperform inexperienced firms. On the contrary, the evidence suggests that the circumstances surrounding a firm's birth permanently conditions what it does throughout its life. Moreover, this persistence matters for policy. As Geroski, Mata, and Portugal [2002] point out, while a government may readily intervene to alter current conditions, there is little it can do to change *ex post* the historical circumstances surrounding a firm's birth. Consequently, they argue, greater attention should be directed toward the development of appropriate neonatal policies.

Appendix: From textual histories to data

Translating diverse textual histories into variables for econometric analysis inevitably involves a degree of judgment. This appendix provides a couple of examples to illustrate the process. Others can be found in Thompson [2002].

- In 1844, Thomas Reaney, Jacob Neafie and William Smith formed a partnership in Philadelphia, PA, to build fire engines, boilers and stationary steam engines. However, in that year they also launched four iron steamboats destined for export to South America (Morrison [1905]). Smith died in 1845, and Capt. John P. Levy was invited to join the firm. While Neafie and Levy were experienced mechanics, their social connections and financial resources were limited. Levy brought connections and money from his shipping activities, which facilitated the firm's subsequent expansion. In 1859, Reaney left the firm and established a yard in Chester, PA, in partnership with his son (Heinrich [1997]).

I treated the Philadelphia plant owned by various permutations of Reaney, Neafie, Smith and Levy, as a single firm created in 1844. This was consistent with a general practice of not coding as firm changes the many recorded instances when partners left firms and were replaced by new partners, as long as some key partners remained in place. Although Levy joined the firm after the company had launched its first vessel, the captain brought his connections to the firm at a sufficiently early stage that I include his shipping background as part of the firm's pre-entry experience. The Chester firm formed by Reaney and his son in 1859 is a new spin-off with metal shipbuilding as a pre-entry background, but the loss of Reaney from the Philadelphia partnership did not persuade me to code the surviving partnership of Neafie and Levy as a new firm.

- In July 1885, John Roach and Sons of Philadelphia, PA, declared bankruptcy in the wake of financial strains that arose after underbidding on naval contracts. The yard closed for almost a year, putting 1,200 men out of work. Some negotiations allowed the receiver, George Quintard, to finish some incomplete cruisers on the stocks and ensure payment for them. The following year, after incorporation as the Delaware River Iron Shipbuilding and Engine Works in a reorganization involving new investors, the yard re-opened. The father, John Roach, who was by now terminally ill with cancer, resigned, and the yard re-opened with the son, John B. Roach, at the helm (Heinrich [1997], Swann [1965]).

In this case, I exercised a decision to code any reorganized firm after bankruptcy as a new and distinct firm, but coded the pre-entry background of the new firm as metal shipbuilding.

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