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RESEARCH ARTICLE

Did victories in certification contests affect the survival of organizations in the American automobile industry during 1895–1912? A replication study

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***Research summary*:** We successfully replicate the highly influential study: "The social construction of reputation …,"(Rao, 1994) which reports that cumulative victories in certification contests are negatively associated with firm failure. The replication is robust to the inclusion of additional controls. As in the original, tests of whether the theory is most powerful under higher uncertainty are not supported. Further, placing second, third, or merely participating in races also negatively predicts firm failure, and there is insufficient information in the data to tease out the importance of these predictors versus race victories. We discuss the assumptions under which the evidence can be interpreted as supportive of a more general argument of "loose coupling", where affiliation with certification contests reduces firm failure.

***Managerial summary*:** We successfully replicate a study that related victories in races to the survival of early automobile firms. This result was interpreted as evidence that rank-order certification contests legitimized firms and led to survival. We then demonstrate that there is insufficient information to tease out the relative importance of victories, as opposed to placing second, third, or merely participating in predicting survival. Our result is consistent with an argument that affiliation with certification contests, not only winning them, increases a firm's chances of survival. It is also consistent with an argument that firms with better quality automobiles won races and survived. An implication of our work is that there is insufficient evidence to determine if firms in new industries should expend finite resources on participation in certification contests or improvement of product quality.

KEYWORDS
automobiles, certification contests, legitimacy, replication, reputation

1 | INTRODUCTION

Recently, management researchers have questioned our field's "biases against…replication studies" (Bettis, 2012: 108) and highlighted the importance of replication for the "empirical credibility of strategic management scholarship" (Bettis, Helfat, & Shaver, 2016: 2193). As Bettis noted, without revisiting existing empirical evidence, "the theory we are building may be more like a house of cards than a strong and enduring edifice of tightly welded steel beams" (Bettis, 2012: 111). Indeed, Goldfarb and King (2016) find that we should expect approximately one in four results not to replicate. To address these concerns, *Strategic Management Journal* has recently changed its editorial policies to explicitly encourage replications with the goal of providing "additional evidence that helps to build a cumulative body of knowledge in strategic management" (Ethiraj, Gambardella, and Helfat, 2016: 2191).

In line with these developments, we replicate an influential study published in the *Strategic Management Journal*: "The social construction of reputation: Certification contests, legitimation, and the survival of organizations in the American automobile industry: 1895–1912" by Hayagreeva Rao (1994). This paper conjectures that "victories in certification contests legitimate organizations and enable them to create favorable reputations" (Rao, 1994: 30). Favorable reputations, in turn, improve winning organizations' "access to resources, and, thereby enhance their survival prospects" (Rao, 1994: 32). Empirically, the study shows a negative and statistically significant association between accumulated victories in car races and exit hazards of car manufacturers, in support of the central conjecture. Based on the argument that legitimation should be more important for new firms, the study also includes tests of the moderating effect of victories and startup firms on firm exit, but does not find empirical evidence in support of this premise.

As of January 2018, the paper had over 1400 Google Scholar citations and over 500 citations in the Web of Science by scholars in a variety of disciplines including strategy, sociology, psychology, tourism, and human resources (e.g., Baum & Powell, 1995; Ferris et al., 1998; Lounsbury & Glynn, 2001; Medina-Muñoz & García-Falcón, 2000; Tyler, 2006). In 2017, the paper garnered 104 additional citations.[1] In management research, the paper is often the starting point of studies that examine how an organization's social approval assets, such as legitimacy and reputation, affect its performance metrics, such as profitability and survival. Out of 91 recent or highly-cited management papers that cite this study: 17 cite the paper for finding a relationship between victories in certification contests and legitimacy or reputation; 29 cite it for finding a positive association between legitimacy or reputation and firm performance; 17 papers cite it for finding the direct effect of victories in certification contests on firm performance; and 33 studies cite the paper for its general methodology, context, or broader theoretical claims.[2] That is, the paper is generally cited to support the premise that firm survival is related to industry legitimation and firm's reputational gains. Overall, although the paper was published 24 years

[1]Google Scholar search, January 24, 2018.

[2]To analyze articles that cited Rao's 1994 study, we used Web of Science. We limited our search to academic articles published in prominent management journals (*Administrative Science Quarterly*, *Academy of Management Journal*, *Academy of Management Review*, *Management Science*, *Organization Science*, *Research Policy*, and *Strategic Management Journal*). To make the task manageable, we specifically focused on those studies that (1) were cited at least 50 times in the Web of Science and/or (2) were published after 2009 and had at least 20 citations in a prominent management journal. We found 91 articles that fit these criteria.

ago, it continues to have a substantial influence on knowledge accumulation in different research domains. To replicate this study, we reconstructed the dataset from the original data sources referenced in the paper and reran the analyses used in the original study. While our data and sample are not strictly the same as the ones used in the original study, we use identical sources and have a much more comprehensive sample drawn from the same population of races, as opposed to a new sample of similar size.

In the language of Bettis, Helfat, and Shaver (2016), this constitutes a "narrow replication". The results of our replication are in line with the original study's findings in support of its Hypothesis 1: we find a robust negative relationship between victories in car races and firm exit. As in the original study, this relationship is not stronger for startup firms, where, according to Hypothesis 2, the proposed theoretical predictions should be most pronounced.

To examine the reasons for the lack of support for Hypothesis 2 in the original study and in our replication, we then proceed with what Bettis, Helfat, and Shaver, 2016 call a "quasi-replication". We use an extended research design to check "robustness to different measures, methods, and models" (Bettis, Helfat, and Shaver, 2016: 2196). First, we use an alternative measure of startups and continue to find no support for Hypothesis 2. Second, we test whether the theoretical framework developed in the original paper can help predict additional patterns in the data. The original paper theorized that race victories were more likely to lead to greater reputational benefits and therefore survival under conditions of high uncertainty. Hypothesis 2, therefore, proposes that the effects of cumulative victories on survival are stronger for startups versus lateral entrants. In line with this idea, we test whether certification contest victories during early stages of the industry, recent victories, and victories by firms from different cohorts led to greater legitimation and reputational benefits and, as a result, were more likely to be associated with firm survival. However, we find no support for these predictions.

To further explore whether the hypothesized model maps to the processes that generated the association between victories and survival in the data, we collected information on second and third places in races as well as race appearances. Once we control for placing second, third, or appearing in a race, victories no longer predict firm exit. We cannot reliably separate the association between winning races and survival as opposed to placing second or third or merely participating and survival due to a high correlation between victories and these three variables (the lowest correlation is 0.89). This implies that there is very little information in the data to distinguish between the survival benefits of winning races versus placing second, third, or merely participating in them. In this sense, the strict interpretation of the specific hypothesis developed in the original study, that victories in certification contests lead to reputational benefits and therefore survival, is not supported by the data in our replication. Yet, this correlation between race participation and survival is consistent with a broader interpretation of some of the theoretical arguments developed in the original study: affiliation with certification contests (i.e., victories, placing second, third, or merely participating) negatively predicts firm exit.

The empirical exercise in the original study implicitly assumes that entry into races is a random event. However, the decision to enter a race is an endogenous choice. Firms with better quality automobiles may have been most likely to win races and survive. We try to address this endogeneity problem by directly controlling for firm quality with several variables, including the number of produced cars and spinoff heritage. After including these and other variables in our analyses, we continue to find results consistent with Hypothesis 1, that victories robustly predict firm survival, and we continue to not find results consistent with Hypothesis 2.

This replication exercise leads to a twofold contribution. First, the narrow replication and parts of the quasi-replication are successful. We replicate the results of the original study and demonstrate that they are robust to the inclusion of more direct controls for firm quality. Second, in our quasi-replication where we conduct a series of robustness tests, we find that placing second, third, or merely participating in certification contests correlates with survival just as much as winning them. This result is consistent with a more general argument that affiliation with certification contests, not only winning them, increases a firm's survival prospects. However, as we elaborate in the Discussion this interpretation is predicated on several strong assumptions.

1.1 | Replication exercise

We begin our replication exercise with a review of the theoretical arguments and hypotheses developed in the original study. The study is motivated by the aphorism that both reputation and legitimacy are important for the survival of organizations. Rao wrote that whereas the reputation literature highlighted tight coupling between signals based on past actions and future expectations, the legitimacy literature highlighted “loose coupling between the endowments of organizations and their credibility” and emphasized the importance of obtaining environmentally preferred symbols (Rao, 1994: 30). He proposed that if both signals and symbols were important “to create impressions on audiences,” then reputation could be understood as “the product of social construction and validation” (Rao, 1994: 30–31). The study thus postulates that reputation is a result of the process of legitimation. Yet, as Rao pointed out, there was, at the time of the study's publication, little empirical evidence that examined “how legitimation generate[d] a favorable reputation and improve[d] the survival of organizations” (Rao, 1994: 31).

To address this, Rao proposed that certification contests helped legitimate organizations and provide them with opportunities to “create favorable reputations” (Rao, 1994: 30). He highlighted the importance of certification contests, or “yardstick competition[s] in which performances are ranked,” for helping evaluate products along “preset criteria” (Rao, 1994: 31–32). These contests may “induce artificial distinctions between equivalent participants,” and, therefore, victories in certification contests are “small, fortuitous events that create a reputation” (Rao, 1994: 32). The reason why victories in such contests help “legitimate organizations and validate their reputation” is “because of the taken for granted axiom that winners are ‘better’ than losers” (Rao, 1994: 32). Reputational gains obtained from cumulative victories in certification contests, in turn, improve survival prospects of organizations. The first and only supported hypothesis of the paper therefore states:

> **Hypothesis 1 (H1)** The greater the natural logarithm of the number of cumulative victories accumulated by organizations in product certification contests, the greater is their reputation, and the lesser is their exit rate.

The second hypothesis follows from Hypothesis 1 and predicts that the negative effect of victories in certification contests on exit rates will be stronger for startup organizations. As the author writes, this is because startups face more uncertainty and higher external liabilities of newness and “victories in contests are likely to improve the reputation of startup organizations to a greater extent than those of lateral entries” (Rao, 1994: 33). Formerly stated:

> **Hypothesis 2 (H2)** Cumulative victories decrease the exit rate of startup organizations more than those of lateral entries.

Empirically, the study examines the association between the number of accumulated victories in car racing contests and car manufacturers' exit rates in the U.S. automobile industry during 1895–1912. The hypotheses in the original study were evaluated in the context of automobile races during the selected period because “it was a new organizational form characterized by considerable ambiguity caused by the lack of standards” (Rao, 1994: 33). As the automobile developed as a form of transportation, auto enthusiasts started organizing racing contests, the public started attending the races, and the magazines started covering the races. The contests "enhanced reputations of individual firms" and "winning firms reaped substantial publicity because of press coverage" (Rao, 1994: 34). The study finds a negative association with a low p-value between cumulative victories and exit rates, but also that this association is not stronger for startup firms.

1.2 | Narrow replication

1.2.1 | Sample

We began our sample construction by collecting the data from the original sources used in the 1994 study. Specifically, we relied on the *Horseless Age* magazine for an inventory of car races. Whereas the final sample in the original study consisted of 381 car races, we found and coded 1451 unique car races, held under the auspices of 305 tournaments in which 444 unique car manufacturers participated between 1895 and 1912. Consistent with the approach in the original study, we defined an organization as an entity that manufactured cars for sale and did not include in our sample "lone inventors who made experimental cars" (Rao, 1994: 36). Based on our personal correspondence with the author of the original study, the original study excluded races that did not report victors: 151 of the 1451 races we coded did not include victors, which we include when we consider the relationship between victories and survival.[3] This allows a more robust examination of the theories predictions.

To obtain yearly data on car manufacturers that produced and sold cars during the period of the study, we followed the guidelines in the original study and used the *Standard Catalogue of American Cars 1805–1942* (henceforth, *Standard Catalogue)* as the main data source. Although the study explicitly excluded lone inventors producing experimental cars, the *Standard Catalogue* also includes entrants that were not lone inventors but still may not have actually produced cars that were easy to buy. To verify whether each producer actually produced cars, we, along with three research assistants, read descriptive entries in the *Standard Catalogue.*

To further ensure that our sample is made up of entrants who were producing and offering products sold in the market, we followed Raff and Trajtenberg (1996) and cross-checked with two additional periodicals, *Automotive Industries* and *Motor*, which listed manufacturers displaying cars in the National Automobile Show in New York. The first such auto show was held in 1900, and each of the 48,000 visitors paid a steep $0.50 fee for entry. In 1901, 1,000,000 people visited the New York Automobile Show (see Flink, 1972).[4] The show remains the premier auto show to this day.

Overall, our threshold for entry is higher than in the original study—we require that a product was not only produced, but also offered for sale. The survival advantage of winning producers over non-winning producers is likely smaller than the survival advantage of winning producers over non-winning experimental non-producers. Therefore, excluding experimental non-producers should make our tests more conservative. This effort resulted in a dataset of 707 unique verified car manufacturers and 2120 manufacturer-years for the period between 1895 and 1912. The median survival of these firms was 2 years with 63% of them failing during the period of our study.

The original study reports 4124 manufacturer-years, almost twice as many as in our sample. To ensure that we have a comprehensive dataset and that our result is not dependent on our more restrictive criteria for inclusion, we complemented our dataset with data generously shared by scholars who have studied the early U.S. automobile industry. First, a series of papers starting with Carroll,

[3]Personal correspondence between the authors and Huggy Rao. November 2016 and July 2017. We are unsure why we were able to identify a greater number of races.

[4]http://aoghs.org/editors-picks/first-auto-show/, viewed March 19, 2016.

Bigelow, Seidel, and Tsai (1996, see also a series of papers by Dobrev & Bigelow) use data from the *Standard Catalogue*. In a separate effort, Ioannis Ioannou (2014) constructed a dataset, similar to that of Steven Klepper, that tracked spinoffs in U.S. automobile industry.[5] We refer to these datasets as the "Dobrev" dataset and the "Ioannou" dataset, respectively. We cross referenced our data with these two datasets and found 469 firms that were in either the Dobrev or Ioannou datasets but not in our sample of verified manufacturers. The unified dataset, which includes the Dobrev and Ioannou datasets and relaxes the restrictions of entry into the New York Automobile Show, includes 1176 unique manufacturers and 3191 manufacturer-years.[6] Note that per the discussion above, this number drops to 2120 once we screen for actual evidence of manufacturing a car. In the analyses below we use the more restricted sample, though the results are very similar if we use the more expansive one.

A median length of firm survival in this dataset is also 2 years, with 72% of firms failing during the period of our study. Of the identified 1176 manufacturers, 168 firms participated in races. The median firm that participated in a race survived 4.5 years and 35% of these failed during our time period. Thus, the basic result that racing firms survive longer is evident in the summary statistics. In addition, the relationship between victories and survival is robustly found in this sample.

1.2.2 | Variables

As in the original study, we measured *Firm exit* as cessation of operations and we did not define mergers as exits. Following Klepper (2007), we relied on Smith (1968) for a record of mergers and acquisitions and supplemented this information with descriptions in the *Standard Catalogue* where necessary. If there was no record of an acquisition in either of these sources, we recorded cessation of operations described in the *Standard Catalogue* as an exit. Following the original study, we measured *Victories* as the natural logarithm of one plus the cumulative number of contests won by a car manufacturer from 1895 up to a given year. This measure was lagged by 1 year, as in the original study. We also created a *Startup* variable to account for de novo organizations, which were expected to have a higher exit rate. We coded this variable from the origin stories in the *Standard Catalogue*, and also cross-referenced our coding with the Ioannou dataset.

To measure whether a firm produced gasoline powered cars, we set a dummy variable, *Gasoline technology*, equal to one if, according to the *Standard Catalogue*, a manufacturer produced these types of cars irrespective of whether the said manufacturer also produced cars using electric or steam technology, and zero otherwise. The original study controlled for age and age-squared, which is picked up in the baseline exponential hazard parameter (see below). The original study lacked a measure of size, so it used the firm's initial capitalization as a control (this is sometimes reported in the *Standard Catalogue*). Following the original study, we collected and used the logged form, *Ln [Capital]*. We supplemented data on a firm's starting capital from the *Horseless Age* magazine, when it was available. Following the original study, we included first and second-order measures of *Population density*, the natural logarithm of the number of *Accumulated contests* across the industry, first and second-order measures of *Prior exits*, and we substituted the deflated per capita income variable used in the original study with the deflated *GDP* per capita, as these data were more readily available. Following the original study's methodology, we lagged all independent variables by 1 year.

[5]We thank Stanislav Dobrev and Lyda Bigelow as well as Ioannis Ioannou for generously sharing their data. At the time of this writing, Steven Klepper's estate had yet to make his data available. However, Ioannis Ioannou cross-referenced his data with Klepper's, so the datasets are very similar.

[6]Our dataset includes an exhaustive examination of the *Standard Catalogue* for evidence of any manufacturing activity as well as a comparison and integration of datasets used by other scholars. We are unsure about the source of the continued discrepancy in the number of observations.

TABLE 1 Descriptive statistics and correlations reported in the original study

	Variable	1	2	3	4	5	6	7	8	9
1	Gasoline technology	1.00								
2	Startup	−0.04*	1.00							
3	Age	0.02	−0.04*	1.00						
4	Population density	0.23*	−0.05*	0.15*	1.00					
5	Income	0.23*	−0.04*	0.18*	0.85*	1.00				
6	Prior exits	0.18*	−0.04*	0.13*	0.77*	0.64*	1.00			
7	Log of accumulated victories	0.13*	−0.05*	0.42*	0.16*	0.19*	0.13*	1.00		
8	Log of automobile sales	0.25*	−0.06*	0.21*	0.90*	0.88*	0.70*	0.21*	1.00	
9	Log of capital	0.08*	0.04	0.10*	−0.00	0.01	0.02*	0.37*	0.01*	1.00
10	Log of accumulated contests	0.25*	−0.05*	0.19*	0.92*	0.90*	0.79*	0.19*	0.94*	0.01

Note: Correlations of variables with capital pertain only to the sub-sample with size data.

*$p < 0.05$.

We report the descriptive statistics and correlations from the original study in Table 1 (means and standard deviations were not reported for all variables in the original paper). We also report the summary statistics and correlations for variables we use in the replication study in Table 2. In general, the correlations in the original and current study are similar. Older firms are more likely to win races and this correlation is large in both the original and replicated datasets (0.42 and 0.56, respectively). As in the original study, we find correlations between the natural logarithm of accumulated victories and other measures correlated with the size of the industry, such as the natural logarithm of automobile sales (0.21 in the original, 0.19 in the replication) and prior exits (0.13 in the original, 0.14 in the replication).

We also note some differences in terms of the correlation between victories and other independent variables. For example, the natural logarithm of victories is positively correlated with the production of gasoline powered cars in the original study ($\rho = 0.13$), though this correlation is negative in our replicated dataset ($\rho = -0.04$). The original study's reported correlation of 0.37 between starting capital and victories is much higher than our measured correlation of 0.00. Nevertheless, consistent with the theoretical premise of the study, we find a correlation of −0.20 between race victories and firm failure in the subsequent year.

1.2.3 | Analysis

To enhance transparency, we provide our data and code online. The original study reported exponential hazard models, which we do as well. Our results are robust to other estimation techniques.[7] We report hazard ratios. Estimation was done with Stata 15 using the *streg* routine and the exponential option. We present the results of our replication of all models reported in the original study in Table 3. In this table, the model labels correspond to our replications of similarly labelled models in the original paper. For comparison, we also report regression results of Models 3–2 and 3–3 from the original study (Original M2, Original M3)—which are the most direct tests of Hypotheses 1 and 2. To better aid interpretability, we report hazard ratios and 95% confidence intervals and transform the coefficients and standard errors reported in the original study. Overlapping confidence intervals suggest results are consistent between the two studies.

[7]A Cox model is inappropriate, because the proportional hazards assumption is not consistent with the data. Our central result regarding the correlation between cumulative victories and firm failure is obtained with accelerated time models, i.e., distributional assumptions including Weibull, Log Normal, and Log Log.

TABLE 2 Descriptive statistics and correlations in the replication study[a]

	Variable	Mean	S.D.	Min	Max	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
1	Ln(victories+1)	0.41	0.93	0.00	4.11	1.00																				
2	Ln(second place finishes+1)	0.35	0.81	0.00	3.83	0.95	1.00																			
3	Ln(third place finishes+1)	0.26	0.63	0.00	3.87	0.89	0.91	1.00																		
4	Ln(appearances+1)	0.72	1.32	0.00	5.32	0.94	0.93	0.90	1.00																	
5	Total industry production (t + 1)	4.84	2.21	0.00	12.04	0.50	0.52	0.51	0.58	1.00																
6	Firm exit	0.23	0.42	0.00	1.00	−0.20	−0.20	−0.19	−0.23	−0.43	1.00															
7	Gasoline technology	0.86	0.35	0.00	1.00	−0.04	−0.03	−0.03	0.00	−0.05	0.06	1.00														
8	Startup	0.66	0.47	0.00	1.00	−0.21	−0.20	−0.20	−0.26	−0.13	0.14	0.01	1.00													
9	Startup X Ln(victories+1)	0.18	0.63	0.00	4.11	0.61	0.59	0.58	0.58	0.32	−0.11	−0.01	0.21	1.00												
10	Firm age	3.04	2.60	0.00	17.00	0.56	0.55	0.56	0.60	0.47	−0.21	−0.14	−0.16	0.31	1.00											
11	Ln(Capital)	1.21	3.59	0.00	16.12	0.00	−0.02	0.00	0.01	0.03	−0.01	−0.02	−0.01	0.05	0.04	1.00										
12	Population density	718.14	178.20	16.00	798.00	0.11	0.10	0.10	0.13	−0.04	−0.05	0.13	−0.15	0.07	0.11	0.05	1.00									
13	Population density ̂2	5,474.72	1,484.04	2.56	6,368.04	0.09	0.09	0.08	0.12	−0.09	−0.04	0.12	−0.15	0.06	0.10	0.04	1.00	1.00								
14	GDP per capita	4,758.69	381.48	3,438.00	5,201.00	0.18	0.19	0.20	0.24	0.18	−0.09	0.16	−0.16	0.12	0.28	0.05	0.68	0.64	1.00							
15	Prior exits	31.28	12.90	1.00	52.00	0.14	0.14	0.15	0.19	0.08	−0.07	0.18	−0.16	0.10	0.22	0.06	0.61	0.59	0.63	1.00						
16	Prior exits ̂2	11.45	7.33	0.01	27.04	0.11	0.11	0.12	0.15	0.05	−0.06	0.15	−0.13	0.08	0.19	0.05	0.40	0.39	0.49	0.96	1.00					
17	Ln(automobile sales)	10.60	2.69	0.00	12.70	0.19	0.19	0.20	0.24	0.24	−0.08	0.18	−0.19	0.12	0.27	0.07	0.86	0.82	0.88	0.80	0.63	1.00				
18	Ln(accumulated contests+1)	2.91	0.98	0.00	3.69	0.17	0.17	0.17	0.21	0.11	−0.07	0.17	−0.17	0.11	0.22	0.07	0.75	0.72	0.70	0.69	0.53	0.86	1.00			
19	Spinoff	0.08	0.27	0.00	1.00	0.11	0.16	0.13	0.14	0.18	−0.06	0.04	0.09	0.25	0.05	−0.01	0.06	0.06	0.09	0.07	0.06	0.10	0.09	1.00		
20	Top-ten parent spinoff	0.03	0.16	0.00	1.00	0.14	0.19	0.17	0.17	0.23	−0.06	0.05	0.08	0.26	0.03	−0.02	0.04	0.03	0.07	0.05	0.04	0.07	0.07	0.55	1.00	
21	Pre-1907	0.44	0.50	0.00	1.00	−0.15	−0.16	−0.18	−0.21	−0.20	0.06	−0.14	0.12	−0.10	−0.32	−0.04	−0.28	−0.25	−0.62	−0.69	−0.68	−0.63	−0.35	−0.08	−0.05	0.08
22	Young	0.56	0.50	0.00	1.00	−0.40	−0.39	−0.39	−0.47	−0.40	0.19	0.07	0.14	−0.25	−0.77	−0.01	−0.12	−0.11	−0.25	−0.19	−0.15	−0.23	−0.20	−0.03	−0.01	0.34
23	Pre-1907 X Ln(victories+1)	0.12	0.50	0.00	3.95	0.46	0.41	0.31	0.40	0.17	−0.11	−0.07	−0.11	0.27	0.06	−0.02	0.06	0.05	−0.02	−0.08	−0.12	0.00	0.10	0.04	0.05	−0.37
24	MI	0.13	0.34	0.00	1.00	0.05	0.07	0.05	0.09	0.18	−0.03	0.09	0.03	0.11	−0.02	0.07	0.09	0.08	0.12	0.09	0.06	0.13	0.13	0.30	0.35	−0.11

TABLE 2 (Continued)

Variable	Mean	S.D.	Min	Max	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
25 NY	0.12	0.33	0.00	1.00	−0.01	−0.02	0.00	−0.02	−0.11	0.05	−0.07	0.09	0.06	−0.04	−0.05	−0.02	−0.01	−0.08	−0.09	−0.08	−0.08	−0.07	−0.02	−0.03	0.03
26 OH	0.12	0.33	0.00	1.00	0.07	0.06	0.04	0.04	0.00	0.00	0.02	−0.08	−0.04	−0.01	−0.03	0.04	0.04	0.00	0.02	0.01	0.02	0.02	−0.08	−0.05	−0.07
27 IN	0.11	0.31	0.00	1.00	0.01	0.00	0.03	0.05	0.10	−0.09	0.02	−0.10	0.04	0.08	0.03	0.07	0.07	0.12	0.12	0.10	0.13	0.11	−0.01	−0.05	−0.04
28 Ln(prior year victories+1)	0.15	0.49	0.00	3.47	0.67	0.64	0.60	0.63	0.33	−0.14	0.01	−0.14	0.43	0.23	0.01	0.08	0.07	0.07	0.08	0.06	0.11	0.15	0.09	0.11	−0.50
29 Total firm production (t-1)	2.19	2.82	0.00	10.46	0.60	0.60	0.60	0.70	0.64	−0.25	0.07	−0.32	0.40	0.56	0.12	0.19	0.17	0.34	0.28	0.23	0.34	0.30	0.15	0.17	−0.59
30 Produced models	0.66	0.83	0.00	3.78	0.53	0.53	0.53	0.62	0.48	−0.20	0.18	−0.31	0.33	0.43	0.08	0.19	0.17	0.37	0.32	0.29	0.37	0.30	0.15	0.13	−0.47
31 Top-ten parent spinoff X Ln(victories+1)	0.03	0.28	0.00	3.56	0.25	0.31	0.27	0.27	0.22	−0.05	0.05	0.08	0.41	0.13	−0.02	0.03	0.02	0.06	0.04	0.03	0.05	0.05	0.38	0.69	−0.18
32 Spinoff X Ln(victories+1)	0.06	0.38	0.00	3.81	0.34	0.38	0.35	0.35	0.24	−0.07	−0.04	0.10	0.55	0.20	−0.03	0.04	0.03	0.08	0.06	0.05	0.08	0.07	0.55	0.49	−0.26
33 Young X Ln(victories+1)	0.05	0.28	0.00	3.61	0.24	0.21	0.13	0.21	0.07	−0.06	0.04	−0.05	0.18	−0.09	−0.01	0.04	0.04	−0.01	0.03	0.02	0.03	0.07	0.04	0.08	−0.27
34 Cohort 1	0.51	0.50	0.00	1.00	0.26	0.24	0.22	0.23	0.17	−0.05	−0.18	−0.01	0.15	0.29	0.01	−0.24	−0.21	−0.52	−0.46	−0.42	−0.49	−0.35	−0.06	−0.02	−0.24
35 Cohort 2	0.37	0.48	0.00	1.00	−0.21	−0.20	−0.18	−0.17	−0.11	0.02	0.14	0.01	−0.12	−0.15	−0.05	0.20	0.18	0.34	0.33	0.29	0.33	0.21	0.02	−0.03	0.16
36 Cohort 3	0.12	0.32	0.00	1.00	−0.10	−0.08	−0.08	−0.11	−0.10	0.05	0.06	0.01	−0.04	−0.23	0.07	0.07	0.05	0.29	0.21	0.22	0.26	0.24	0.05	0.07	0.13

Variable	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36
Pre-1907	1.00															
Young	0.25	1.00														
Pre-1907 X Ln(victories+1)	0.26	−0.14	1.00													
MI	−0.06	0.02	0.00	1.00												
NY	0.10	0.02	0.01	−0.15	1.00											
OH	0.03	0.01	0.06	−0.14	−0.14	1.00										
IN	−0.12	−0.08	−0.02	−0.14	−0.13	−0.13	1.00									
Ln(prior year victories+1)	−0.02	−0.20	0.52	0.05	0.00	0.05	0.01	1.00								
Total firm production (t – 1)	−0.32	−0.53	0.22	0.11	−0.10	0.00	0.12	0.35	1.00							
Produced models	−0.39	−0.36	0.12	0.10	−0.09	−0.01	0.14	0.32	0.72	1.00						
Top-ten parent spinoff X Ln(victories+1)	−0.05	−0.09	0.09	0.29	−0.04	−0.04	−0.04	0.18	0.22	0.15	1.00					
Spinoff X Ln(victories+1)	−0.07	−0.15	0.13	0.21	−0.06	−0.06	0.00	0.24	0.27	0.24	0.73	1.00				

TABLE 2 (Continued)

Variable	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36
Young X Ln(victories+1)	0.06	0.15	0.30	0.04	0.01	−0.01	0.00	0.41	0.04	0.04	0.07	0.05	1.00			
Cohort 1	0.59	−0.19	0.21	−0.06	0.07	0.04	−0.05	0.13	0.08	−0.05	0.05	0.07	0.00	1.00		
Cohort 2	−0.39	0.04	−0.16	−0.02	−0.04	−0.07	0.07	−0.12	−0.01	0.04	−0.05	−0.07	−0.05	−0.79	1.00	
Cohort 3	−0.32	0.24	−0.08	0.12	−0.04	0.04	−0.03	−0.02	−0.12	0.01	0.00	−0.01	0.07	−0.37	−0.28	1.00

[a]Descriptive statistics and correlations of year dummies are available in the released data.

TABLE 3 Narrow replication: Exponential hazard models predicting firm exit based on single year lagged race measures

Variable	3–1	Original M2	3–2	Original M3	3–3	3–4	3–5	3–6	3–7	3–8
Gasoline technology	1.34 [1.00,1.80]	0.725 [0.633,0.832]	1.28 [0.96,1.71]	0.766 [0.667,0.879]	1.28 [0.95,1.71]	1.26 [0.94,1.68]	1.79 [0.62,5.12]	1.80 [0.65,4.94]	1.31 [0.98,1.75]	1.31 [0.97,1.75]
Startup	2.02 [1.59,2.56]	1.201 [1.002,1.44]	1.76 [1.38,2.24]	1.206 [1.006,1.446]	1.69 [1.32,2.17]	1.75 [1.38,2.22]	2.14 [1.04,4.40]	2.19 [1.07,4.48]	1.74 [1.37,2.21]	1.67 [1.30,2.14]
Age		1.128 [1.05,1.212]		1.128 [1.05,1.213]						
Age ˆ2		0.393 [0.215,0.72]		0.392 [0.214,0.719]						
Ln(capital)							1.03 [0.91,1.17]	1.02 [0.90,1.15]		
Population density	1.00 [0.99,1.02]	0.999 [0.984,1.014]	1.00 [0.99,1.02]	0.999 [0.984,1.014]	1.00 [0.99,1.02]	1.00 [1.00,1.00]	0.97 [0.92,1.03]	1.00 [1.00,1.00]		
Population density ˆ2	1.00 [1.00,1.00]	1.000 [0.998,1.003]	1.00 [1.00,1.00]	1.000 [0.975,1.027]	1.00 [1.00,1.00]		1.00 [1.00,1.01]			
Prior exits	0.94 [0.89,0.99]	1.039 [0.987,1.094]	0.94 [0.89,1.00]	1.040 [0.987,1.094]	0.94 [0.89,1.00]		1.01 [0.82,1.24]			
Prior exits ˆ2	1.09 [1.01,1.18]	0.976 [0.939,1.015]	1.08 [1.00,1.16]	0.975 [0.938,1.014]	1.08 [1.00,1.16]		0.97 [0.73,1.30]			
GDP per capita	1.00 [1.00,1.00]	1.00 [0.998,1.002]	1.00 [1.00,1.00]	1.000 [0.998,1.002]	1.00 [1.00,1.00]	1.00 [1.00,1.00]	1.00 [1.00,1.00]	1.00 [1.00,1.00]		
Ln(automobile sales)	1.06 [0.84,1.34]	1.136 [0.905,1.424]	1.10 [0.87,1.38]	1.155 [0.921,1.449]	1.10 [0.87,1.38]		1.39 [0.72,2.67]			
Ln(accumulated contests+1)	1.00 [0.82,1.21]	0.788 [0.605,1.026]	1.02 [0.84,1.24]	0.788 [0.605,1.026]	1.02 [0.84,1.24]	1.01 [0.87,1.17]	1.07 [0.51,2.26]	1.09 [0.59,2.03]		
Ln(victories+1)		0.414 [0.311,0.551]	0.46 [0.36,0.59]	0.442 [0.259,0.753]	0.37 [0.22,0.61]	0.46 [0.36,0.59]	0.30 [0.12,0.78]	0.30 [0.12,0.77]	0.47 [0.36,0.60]	0.38 [0.23,0.62]
Startup X Ln(victories+1)				0.917 [0.484,1.736]	1.38 [0.77,2.48]					1.39 [0.78,2.48]
Year fixed effects									Included	Included
Observations	2120	4124	2120	4124	2120	2120	242	242	2120	2120

Note: Exponential failure hazard models. Hazard ratios reported. 95% confidence intervals in parentheses. Model numbers replicate original paper specifications.

As can be seen from Table 3, the 95% confidence intervals overlap for the startup indicator, population density and its square, prior exits and its square, measures of income and market size (*Ln[Automobile sales]*). We note that the original study reports per-capita income, and we use per-capita gross domestic product in our regressions. In our replication study, gasoline car manufacturers appear more likely to exit, though in the original study the results of some analyses suggest a lower hazard of exit for gasoline manufacturers. In the exponential hazard regression, the age variable is measured in the baseline hazard parameter. Because we have poor age information for *de alio* entrants into the automobile industry, we assume age is equal to zero when each firm enters. Because of this, including age as an explanatory variable is superfluous as the effect of age is measured as a non-linear function in the exponential hazard regression. The original study reports a mean age of 3.29 and standard deviation of 2.75, whereas in our sample we find a mean of 3.04 and 2.60 for verified manufacturers.

The estimated hazard ratio for log of cumulative victories (*Ln[Victories + 1]*) is remarkably similar to that in the original study. Our point estimate of the hazard ratio in Model 3–2 is 0.46 (95% confidence interval [0.36, 0.59]), whereas the hazard ratio in the original study is 0.41 (95% confidence interval [0.31, 0.55]). The confidence intervals are almost exactly the same! In our replication of Model 3–3, we find the hazard ratio to be 0.37 [0.22, 0.61], whereas the original study reports it to be 0.44 [0.26, 0.75]—and again the confidence intervals overlap with remarkable precision. Thus, we reproduce the results of the original study that are consistent with Hypothesis 1.

As in the original study, our findings are inconsistent with predictions of Hypothesis 2. The 95% confidence interval in the original study's estimate of *Startup X Ln(Victories + 1)* suggests that the effect may be either positive or negative, and we find the same. In the original study, the 95% confidence interval includes hazard ratios from 0.48 to 1.74, and in our replication, the range is from 0.77 to 2.48. Importantly, our replicated non-finding is a successful replication of the original non-finding.[8]

Following the original study, in Model 3–4 we explore the robustness of the results to the omission of the population ecology controls. Estimated coefficients are stable in both magnitude and precision across these specifications. This is similar to the original study, in which the results are consistent across all specifications (see original study, Table 2). That is, the differences and similarities we find between the original study and the replication models are consistent in Model 3–2, Model 3–3, and Model 3–4.

An ideal test of the main theoretical prediction in the original study would randomly assign race victories to firms. The concern is that victories might accrue to more capable firms, in which case victories would pick up a quality effect because we might expect capable firms to survive longer than incapable firms, irrespective of winning races. Indeed, this is recognized in the original paper that includes founding capital (*Ln(Capital)*) as a control in the robustness tests—based on the well documented idea that firms larger at founding tend to outperform smaller firms (c.f., Mata, Portugal, & Guimaraes, 1995). This information is only sporadically reported in the *Standard Catalogue.* Adding this control results in a sharp reduction of observations, and does not affect the original study's results. The original study reports these regression results in Table 2, Models 5 and 6. For space considerations, we omit them because they report results very similar to Models 3–2 and 3–3. In our replication (Table 3, Models 3–5 and 3–6) the original result is robust to this control (hazard estimate of

[8]As is well known, evaluating interaction terms in non-linear models is non-trivial (Zelner, 2009). However, we can also evaluate the magnitude of interactions by directly interpret the odds ratio of this interaction (Buis, 2010, and see Norton, Wang and Ai, 2004 for a mathematical proof). This does imply a slightly different question. Instead of asking how much the failure hazard increases for the interacted subpopulation, we are asking how are the proportions different. In this case, we are asking how many failed startups given a number of cumulative races should we expect to see as compared to failed de-alio firms with the same cumulative races. Given the non-result nature of this finding, this is appropriate for our discussion.

0.30 with a confidence interval of [0.12, 0.78]). As in the original study, we omit observations with missing initial capitalization values and as such, the sample of Model 3–5 and Model 3–6 is much smaller than that of Models 3–1 and 3–4. The confidence intervals overlap, and *Ln(Capital)* is not predictive of greater survival. This is not surprising given that the measure of *authorized* capital is based on public announcements in the press or other sources. It is not necessarily the case that the authorized capital was actually collected and invested (see Epstein, 1928 for rich detail of the financing of early automobile manufacturers).

Given that the changing industry conditions as proxied by standard population ecology measures are not the central interest of the theory, we substitute these measures with year fixed effects to provide more complete controls for changing conditions across time. There were very few firms and exits prior to 1901, so we include dummies for each year during 1901–1912, leaving the 1895–1900 period as the omitted comparison group. We report the results with year fixed effects in Table 3, Models 3–7 and 3–8. The coefficients on victories and the interaction of victories with startup status are very similar to those reported in Models 3–2 and 3–3. That is, the results are unchanged when using year fixed effects. Therefore, we use year fixed effects, rather than population level controls, in all subsequent analyses.

1.3 | Quasi-replication: Robustness to different measures, methods, and models

The theoretical rationale behind the main hypothesis of the original study is that victories in certification contests help accrue reputational benefits and reduce uncertainty about firms (Rao, 1994)—a problem likely to be particularly acute for new firms or firms in new industries. As such, the theory should be most powerful when uncertainty about the product or the firm is highest. Therefore, the association between victories and survival should be particularly pronounced in uncertain situations, when firms are most likely to accrue reputational benefits from winning certification contests. This is, indeed, the logic behind the unsupported Hypothesis 2 of the original study. The non-findings related to this hypothesis in the original study and in our replication, raise the concern that the correlation between victories and survival may be attributable to other mechanisms than those hypothesized. Therefore, in search for reasons behind this non-finding, and following Goldfarb and King (2016) who recommend checking if additional predictions of the theory can be found in the data, we proceed with our quasi-replication.

1.4 | Testing for conditions of higher uncertainty

According to the theoretical arguments developed in the original study, reputational benefits that accrue to firms from winning certification contests are a result of legitimation processes. These processes are particularly at play when audiences are uncertain how to evaluate the product. In the case of early stages of the auto industry, as Rao writes, the automobile was perceived by many as a new and dangerous invention and a "plaything of the rich" (1994: 34). Yet, as early as at the beginning of the 20th century the uncertainty surrounding the automobile as a mode of transportation declined, partly due to the legitimating quality of certification contests that endowed some firms with favorable reputations (Rao, 1994).

We therefore consider several variables that should help separate either contests or periods of greater uncertainty, when, according to the theoretical arguments developed in the original paper, the reputational benefits should be the highest. Specifically, we considered an alternative measure of the startup variable, examined the effect of victories during early industry stages, victories in particular contests, and controlled for firm cohort.

1.5 | Startups

Hypothesis 2 of the original study predicted that startups should enjoy greater benefits from race victories than established firms because less is known about new firms. The failure to reject the null hypothesis does not support this idea. It is possible that lack of evidence in support of Hypothesis 2 in the original study is due to the measurement of the *Startup* variable: it is always equal to one if the firm was originally a startup, even several years after the firm in question was founded. An alternative is to use firm age and interact it with victories. This would allow us to determine if the advantage of victories accrues primarily to new, young firms.

To simplify interpretation, we use a dummy variable for firms in their first or second year (*Young*), and an interaction term of this dummy with the log of cumulative victories. We report the results of this test in Table 4, Model 4–1, which includes the interaction of *Young* and *Ln(Victories + 1)*. Young firms are more likely to exit, reflecting the greater uncertainty surrounding their survival. However, the confidence interval for the interaction of *Young* and *Ln(Victories + 1)* is very large, indicating that we cannot conclude that younger firms are less likely to exit when winning

TABLE 4 Quasi-replication: Testing robustness to different measures, methods, and models

Variable	4–1	4–2	4–3	4–4	4–5	4–6	4–7
Gasoline technology	1.30 [0.97,1.75]	1.30 [0.97,1.75]	1.36 [1.02,1.82]	1.30 [0.97,1.74]	1.32 [0.98,1.76]	1.31 [0.98,1.76]	1.36 [1.02,1.82]
Startup		1.74 [1.36,2.21]	1.86 [1.46,2.36]	1.70 [1.34,2.17]	1.72 [1.35,2.19]	1.73 [1.35,2.20]	1.60 [1.25,2.03]
Ln(victories+1)	0.50 [0.38,0.66]	0.49 [0.37,0.64]		0.51 [0.39,0.66]	0.53 [0.40,0.70]	0.63 [0.40,1.00]	
Young	1.40 [1.13,1.73]						
Young X Ln(victories+1)	0.71 [0.32,1.56]						
Pre-1907		1.46 [0.93,2.29]					
Pre-1907 X Ln (victories+1)		0.84 [0.45,1.58]					
Ln(prior year victories+1)			0.37 [0.23,0.60]				
Cohort 1 (pre-1905)				0.57 [0.35,0.93]			
Cohort 2 (1905–1909)				0.73 [0.50,1.07]			
Models appearing in a race, mean					0.67 [0.41,1.08]		
Ln(second place finishes+1)						0.73 [0.42,1.29]	
Ln(third place finishes+1)						0.88 [0.48,1.60]	
Ln(appearances+1)							0.57 [0.49,0.67]
Constant					0.14 [0.09,0.22]	0.14 [0.09,0.22]	
Observations	2120	2120	2120	2120	2120	2120	2120

Note: Exponential failure hazard models predicting firm exit with year fixed effects.

races. Consistent with the original study's lack of support for Hypothesis 2, this alternative test also provides no evidence in support of this hypothesis.

1.5.1 | Early stages of the industry

If industry legitimation were the mechanism through which car manufacturers increased their survival chances, we would expect the effects of victories to be larger in the early stages of the industry. This is a time when uncertainty is highest; and when uncertainty is high, potential customers might be looking for external signals of quality such as race victories (Rao, 1994). We thus tested whether there was a positive effect of victories prior to the market crash of 1907 and report the results in Table 4, Model 4–2. We include an interaction of a *Pre-1907* dummy with victories. We find no evidence that winning races led to a special advantage in the early years of the industry.

1.5.2 | Contest characteristics

Victories in some contests may have been more influential than in other contests for firms' reputations and survival chances. To account for contest characteristics, we investigated whether winning more prestigious contests, contests with greater attendance, and recent contests would lead to greater survival. We coded as prestigious those contests that were in top decile based on the number of participating makes relative to other contests in a calendar year. Contests not in the top decile were considered not prestigious. To assess contest attendance, we turned to the *Horseless Age.* Although the magazine did not provide attendance information for all races, we coded it where available (457 races or 29% of our sample). We took the natural logarithm of one plus the number of people who attended all races the company's cars won cumulatively from 1895 though the prior year. However, the correlation between the natural logarithm of victories and natural logarithm of victories in prestigious races is 0.81 and the correlation between the natural logarithm of victories and cumulative attendance at race victories is 0.85. The same firms that accumulated victories generally, accumulated victories in prestigious and well-attended races. Therefore, there is insufficient information in the data to separate these effects.[9]

In the rapidly changing market of the early stages of the automobile industry, it is possible that only recent events strongly influenced firms' reputations, and hence the theoretical predictions of the original study may be strongest when examining the effect of recent victories. We repeated the analyses using as a predictor variable contests victories in the prior year *Ln(Prior year victories + 1)* (i.e., not cumulative from year 1895). We report the results of this analysis in Table 4, Model 4–3. Our results are very similar to those reported in the original study.[10]

[9] In other unreported analyses, we accounted for the type of a contest, by including categorical variables for races that tested “strength” (endurance races, reliability contests, hill climbing contests) and a separate variable for those that tested speed. There are 751 speed races, 299 climbing races, 33 endurance races, and 9 reliability contests. We combined the latter three types into a single group. The results are robust to omitting the two smaller groups (endurance and reliability) or including them separately. The correlation between cumulative victories in speed contests and cumulative victories in strength contests is 0.42. Cumulative victories in strength contests predict survival robustly as do cumulative victories in speed contests, though this has little additional meaning because cumulative victories in speed contests and victories in all contests are correlated at 0.95. When we include both contest types in one model, the effect of victories in speed contest is measured with greater precision and negatively predicts firm failure. Strength races were rarer, and hence this test has less power. We find no evidence that winning particular types of races was beneficial—though we acknowledge that we are uncertain a priori which type of race might have been most useful in building reputation.

[10] In an unreported analysis, we also tested the interaction between recent victories and startups. The results were very similar to the replications reported in Table 3 Model 3–3.

1.5.3 | Cohort

Klepper (2007) predicted that early entrants enjoyed a permanent advantage due to a head start in technology and economies of scale in production. To verify that this is not confounding our results, we follow the econometric specifications of the original study and include cohort dummies. In Table 4 Model 4–4 we include a dummy for *Cohort 1* (those manufacturers that entered in and before 1904) and a dummy for *Cohort 2* (those that entered between 1905 and 1909). We find that earlier entrants survived longer, and the estimated negative correlation between cumulative victories and failure is unchanged. Because cumulative victories and its interaction with the cohort 1 dummy are correlated at 0.93, we cannot reliably interpret an interaction term or isolate whether more reputational benefits accrued to these early entrants as opposed to later entrants.

Overall, in all these tests, we find little evidence that race victories were stronger predictors of firm survival under conditions of higher uncertainty. Given the lack of support for the theoretical arguments behind Hypothesis 2, we reevaluate the relationship between victories and survival to help understand if it is driven by alternative theoretical mechanisms.

1.6 | Victories, placing second and third, and merely participating

The theoretical arguments developed in the original study suggest that reputational advantages accrue to certification contest winners and assist in their survival, hence data on victories appeared sufficient to test this proposition. Yet, as an anonymous reviewer pointed out to us, the broader argument that there is loose coupling between accepted symbols of legitimacy and outcomes would hold that reputational benefits may have accrued to manufacturers whose cars were affiliated with certification contests, regardless of how they performed in races.[11]

As we replicated the data collection and coding process, we noticed that the source of data on car races used in the original study and in our replication exercise, the *Horseless Age*, mentioned not only winners of the races, but also those manufacturers whose cars placed second, third, and often presented a comprehensive list of participants. In addition, multiple vehicles of the same manufacturer often participated in a single race; thus, the same auto manufacturer that was a winner was, at times, simultaneously a loser. With these observations in mind, we proceeded to test whether having multiple models enter a race and whether affiliation with, rather than victory in, car races affected firm exit.

1.6.1 | Multiple entries

To the degree that outcomes are random, a firm with multiple models appearing in a race had a higher chance of winning. In Table 4 Model 4–5 we take into account the fact that some manufacturers' cars appeared multiple times in the same race. We enter both the amount of race entry as well as cumulative victories jointly. Consistent with Hypothesis 1, the effect of cumulative victories on firm exit is robust to the inclusion of this control.

[11]We thank an anonymous reviewer for pointing out the distinction between the specific hypotheses developed in the original study that connect cumulative victories in certification contests and firm survival and more general arguments that affiliation with certification contests may favorably affect firms' survival prospects. We also note that given how the realization of this possibility emerged, we should be cautious in interpreting the result—as it could be considered an attempt to rationalize the theory to inconsistent results. Hence, the possibility of a spurious result is increased. For these reasons, we view our results in this section as more exploratory and theory building than strong factual evidence.

1.6.2 | Second and third places

The Horseless Age reported information on second and third prize winners for 69% of the contests during the studied period. We used these data to measure second and third place finishes in the same way as victories (the natural logarithm of one plus the number of cumulative second place and third place finishes, respectively). In Table 4, Model 4–6, we add second and third place finishes jointly with victories. The confidence intervals indicate that these estimates are not precise. We reject the hypothesis that all three coefficients are jointly zero ($\chi^2_{3d}=33.88, pp=0.00$). However, we cannot reject the hypothesis that first place victories are different than second or third place victories. Note that in Table 2 we find that the correlation between first place, second place, and third place finishes is at least 0.89.

Following Kalnins (2017), in unreported regressions, we ran models without first place finishes, and instead included either second or third place finishes. In each of these regressions, both second and third place finishes strongly and independently predicted a lower hazard of exit. Thus, the data cannot distinguish between the effects of first, second, and third place finishes. We also estimated models that aggregate first, second, and third place finishes. When doing so, the coefficient on the aggregation is negative, with a confidence interval that suggests a negative relationship between placing in the top three positions and survival. Overall, we cannot reject the null hypotheses that differences between coefficients on *Ln(Victories + 1)*, *Ln(Second place finishes + 1)* and *Ln(Third place finishes + 1)* are zero. This implies that there is insufficient information to conclude that placing first was better than placing second or third, or vice versa. Note that this does *not* imply that the difference *is* zero, one cannot conclude that the null is true if one fails to reject it.

1.6.3 | Race appearances

Following the idea that affiliation with certification contests in general, rather than victories in particular, helped firms accrue reputational benefits and secure survival, it is possible that firms may obtain reputational benefits if their cars merely participated in certification contests, and not only won or placed second or third. We measured race appearances as the natural logarithm of one plus the sum of cumulative contests in which a manufacturer's cars appeared from 1895 through a given year. This variable was lagged by 1 year. The *Horseless Age* listed multiple makes that appeared in races 78% of the time and listed all participants in 24% of them. In Figure 1 we see that there is little difference in survival between winners and non-winners conditional on having cars appear in races through age

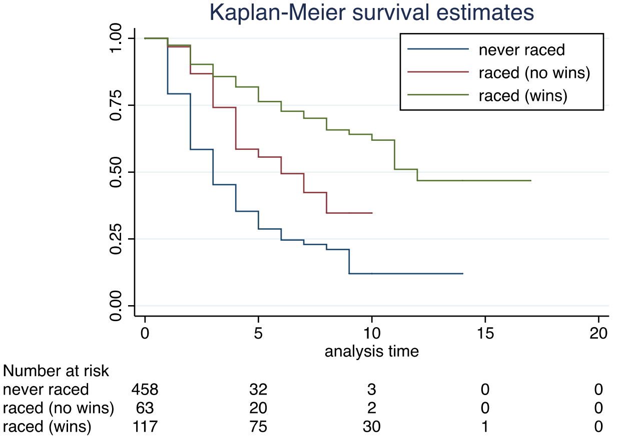

FIGURE 1 Firm survival conditional on appearances and victories in car races

3. After that, there is a higher hazard of failure for those manufacturers whose cars never win a race as compared to those that do.

In Table 4, Model 4–7 we include as the main predictor variable race appearances, *Ln(Appearances + 1)*. We find that appearances strongly predict survival. When we include race appearances and race victories into one regression, the influence of winning a race disappears and the effect loads entirely onto the appearances variable. However, race victories and appearances are correlated at 0.94 and it would be incorrect to interpret the coefficients on variables that are highly correlated if one believes there might be an omitted common factor associated with each of these (Kalnins, 2017). We therefore conclude that there simply is not enough information in the data to inform us whether race victories affected firm survival more or less than did race appearances. Despite our inability to distinguish between the effects of race appearances and race victories, this result is consistent with a broader theoretical argument that assumes a loose coupling and proposes that affiliation with certification contests, rather than victories, legitimizes and therefore increases a firm's survival prospects.[12]

A cautious reader will observe that since neither legitimacy nor reputation are measured directly in the empirical analysis of the original study, additional assumptions are necessary to conclude that affiliation with certification contests led to legitimation and survival. In particular, we must assume that participation is not proxying for product quality or firm capability. This would be an omitted variable bias, i.e., firms whose cars appeared in races were more likely to survive than firms whose cars did not appear in races because they produced better cars. We explore this issue next.

1.7 | Controlling for firm characteristics that predict both, race victories and survival

It is possible that firms with certain characteristics were more likely to win races and survive. We therefore include measures of firm characteristics that may simultaneously predict race victories and survival. To the degree that victories and survival are driven by an omitted third factor, such as strategic and production capabilities or car quality, general measures of firm performance may pick up some or all of the influence of the omitted factor on cumulative victories.

1.7.1 | Firm size

Numerous studies of the U.S. automobile industry suggest that firm size is related to survival, strategic choices, and the competitive advantages that firms achieve (Argyres, Bigelow, & Nickerson, 2015; Carroll et al., 1996; Dobrev & Carroll, 2003). If measures of firm size are correlated with a manufacturer's cars participating in races and account for some measure of firm quality, we should expect the coefficients on victories to lose predictive value as we take into account firm size. In addition, following the logic behind Hypothesis 2 of the original study regarding higher reputational advantages for startups, smaller firms may gain more reputational advantages with winning certification contests.

To assess firm size, we use a measure of production quantities that has been used in prior research (e.g., Carroll et al., 1996; Dobrev, Kim, & Carroll, 2002).[13] Comprehensive production quantity figures come from the Raff-Trajtenberg dataset. This dataset is based upon numbers reported in the

[12]In unreported analyses, we utilize the information on race participation by asking a slightly different question: conditional on a firm ever racing, did cumulative victories predict survival? In our sample, 175 verified manufacturers raced, while 627 did not. Of the 175 firms with cars entered into races, 71 were never victorious. The median time we observe firms who were ever victorious in our sample is 6 years, while 3 is the median for firms who raced but never won. Consistent with Hypothesis 1 of the original study, cumulative victories negatively predict firm failure in this model.

[13]We also considered assessing firm size with sales. We were unable to recover sales data from the Dobrev dataset; however, we expect that production quantity is highly correlated with sales.

Standard Catalogue and other sources for leading firms. While production numbers for leading firms are well known (Smith, 1968), the information for smaller firms is spotty. Sometimes only the total quantity the firm ever produced is known, but how those quantities are distributed across years is not. At other times, even this number is not known with certainty. In the former case, we assumed a distribution with a single production quantity peak and distributed the known total across the years of known activity. In the latter case, the same technique was applied, but to a "guesstimated" number based on the qualitative description in the *Standard Catalogue.* While we acknowledge that this implies a lack of precision, to our knowledge, this is the most comprehensive database of production quantities from the period. We limit this analysis to firms with production estimates in the Raff-Trajtenberg dataset, that is, those that entered the New York Auto Show from 1901 onwards. After coding the production variable, we compared it to the originally used proxy for firm size, initial capitalization, and found that firm production and initial capitalization are correlated only at 0.12. Further, initial capitalization is correlated with firm exit at −0.01, whereas production is correlated with exit at −0.25. This observation suggests that in the context of this study, production may be a better proxy for firm size and, more generally, highlights the importance of using alternative proxies to assess the same construct.[14]

Lagged production and lagged cumulative victories are correlated at 0.60, and moreover, the interaction of lagged cumulative victories and lagged production is correlated at 0.97 with lagged victories. Hence, we cannot meaningfully estimate an interaction effect. Kalnins (2017) makes clear that even identifying the separate effects of production and victories may be challenging if there is an omitted third factor driving both victories and production. In this case, a change in sign or a large change in coefficient magnitude may be a symptom of multicollinearity, as opposed to a real effect. To help diagnose this, Kalnins recommends entering each coefficient individually in separate regressions to provide a baseline for the joint estimation.

In Table 5 Model 5–1 we include lagged production which strongly predicts survival. In Model 5–2 we include both race victories and lagged production. Both strongly predict survival in a joint regression. In unreported regressions, we find that the results are identical when using race participation instead of race victories. This is not consistent with there being an omitted third variable that is confounding our ability to estimate separate effects. Hence under the assumption that conditional on entering the New York Auto Show, production numbers perfectly control for firm quality and strategic acuity, this result is consistent with the prediction that victories and participation in certification contests will increase the likelihood of firm survival.

1.7.2 | Spinoffs and heritage

Klepper (2007) documented that spinoffs outperform other manufacturers in the automobile industry and so reputational advantages stemming from race victories may disappear once we take this variable into account. Ioannou's data, together with our supplementary data collection, allow us to identify spinoffs. In Table 5, Model 5–3 we include *Spinoff.* In addition, following the logic of Hypothesis 2, as compared to de novo entrants, spinoffs might benefit less from victories. In Model 5–4 we include its interaction with victories under the assumption that spinoffs might benefit less from victories as the uncertainty surrounding them is lower than other de novo entrants. We find that confidence interval for the coefficient of the interaction of accumulated victories and spinoffs is large and indicates that the effect may be positive or negative. We note that point estimates of the direct effect of being a spinoff is consistent with the premise of Klepper's theory, though this is not estimated with precision. This may be because it takes time for these advantages to meaningfully affect

[14]We would like to thank an anonymous reviewer for bringing this observation to our attention.

TABLE 5 Controlling for firm characteristics that predict both, race victories and survival

Variable	5–1	5–2	5–3	5–4	5–5	5–6	5–7	5–8	5–9
Gasoline technology	2.02 [0.80,5.08]	1.90 [0.75,4.80]	1.32 [0.98,1.77]	1.32 [0.98,1.77]	1.31 [0.98,1.76]	1.31 [0.98,1.76]	1.32 [0.99,1.77]	1.37 [1.02,1.84]	1.35 [1.00,1.81]
Startup	1.46 [1.00,2.15]	1.52 [1.03,2.23]	1.75 [1.38,2.23]	1.75 [1.37,2.23]	1.75 [1.38,2.23]	1.75 [1.37,2.23]	1.68 [1.32,2.14]	1.63 [1.27,2.09]	1.50 [1.16,1.93]
Ln(victories+1)		0.65 [0.47,0.88]	0.48 [0.37,0.62]	0.47 [0.35,0.62]	0.48 [0.37,0.61]	0.47 [0.36,0.61]	0.47 [0.37,0.61]	0.51 [0.39,0.66]	0.60 [0.46,0.79]
Total firm production (t-1)	0.67 [0.60,0.76]	0.75 [0.65,0.87]							0.91 [0.85,0.97]
Spinoff			0.71 [0.46,1.08]	0.68 [0.43,1.08]					0.77 [0.48,1.25]
Spinoff X Ln(victories+1)				1.16 [0.60,2.26]					
Top-ten parent spinoff					0.57 [0.24,1.39]	0.51 [0.18,1.44]			0.79 [0.29,2.16]
Top-ten parent spinoff X Ln(victories+1)						1.27 [0.49,3.28]			
MI							0.84 [0.61,1.15]		0.91 [0.66,1.27]
NY							1.08 [0.82,1.43]		1.07 [0.81,1.42]
OH							1.02 [0.75,1.37]		0.97 [0.72,1.31]
IN							0.56 [0.37,0.84]		0.58 [0.38,0.88]
Produced models								0.85 [0.72,1.00]	1.01 [0.84,1.23]
Cohort 1 (pre-1905)									0.77 [0.46,1.30]
Cohort 2 (1905–1909)									0.91 [0.61,1.36]
Observations	941	941	2120	2120	2120	2120	2120	2120	2120

Note: Exponential failure hazard models predicting firm exit in Models 5–1 through 5–9 with year fixed effects. OLS with firm fixed-effects predicting firm production in Model 5–10. Hazard ratios reported. 95% confidence intervals in parentheses.

outcomes and our sample period ends in 1912, whereas Klepper's and Ioannou's samples extend much longer.

Klepper attributes success to spinoffs with superior lineage, as measured by spinoffs of previous production leaders. Following this idea, in Model 5–5 we add a variable for a spinoff of a firm that was a top-ten manufacturer in the previous year (*Top-ten parent spinoff*). And in Model 5–6 we interact the top-ten spinoff dummy with cumulative victories. Our results are very similar to those using a regular spinoffs variable.

1.7.3 | Firm location

History shows that better automobile manufacturers were located in just a few states. Thus, in Table 5 Model 5–7 we control for whether a firm was from one of the four states with the biggest concentration of automobile manufacturers (Michigan, New York, Ohio, and Indiana). The correlation between cumulative victories and survival is robust to the inclusion of state controls.

1.7.4 | Number of produced models

The number of models a firm offers on the market may also proxy for quality. It may be difficult to offer many models simultaneously, and this may be indicative of organizational and production capabilities. We include the lagged number of offered models in Table 5 Model 5–8. The association between cumulative victories and survival remains robust to the inclusion of this control.

In Table 5, Model 5–9 we include all quality controls. The coefficient on victories remains remarkably stable and consistent with the results of the original study. Overall, after improving on the methodology of the original study and more directly controlling for firm characteristics, we continue to find evidence consistent with Hypothesis 1 of the original study.

1.7.5 | Additional tests of broader interpretations of the theory

In our final set of analyses, presented in Table 6 (Models 6–1 through 6–6), we evaluate whether there is evidence in support of the broader interpretation of the theoretical arguments developed in the original study: that affiliation with certification contests, that is mere race appearances, legitimizes through loose coupling of signals. Following the logic similar to that which motivates Hypothesis 2, we test whether there is an enhanced impact on survival for startups whose cars appear in races. We report these results in Model 6–1. Similar to the results with victories, we find no support for this prediction.

We then interact appearances with the indicators of higher uncertainty, as we did in Table 4. First, to test whether appearance in races was more likely to lead to firm survival under conditions of higher uncertainty, we substitute Y*oung* for startup and its interaction of appearances in Model 6–2. In Model 6–3 we interact appearances with pre-1907. Second, to more directly control for firm quality, in Model 6–4 we include *Spinoff* and its interaction with appearances, in Model 6–5 we include *Top-ten parent spinoff* and its interaction with *Ln(Appearances + 1)*. Finally, to evaluate whether the correlation between cumulative appearances and survival is robust to other indicators of firm quality, we include spinoff, top-ten parent spinoff, lagged production, state dummies, and number of produced models in Model 6–6 as potential controls for quality. Cumulative appearances strongly predict survival with these additional quality controls. This is also true if we include any of these quality controls individually, though we do not present these results in the table for brevity. Overall, across these regressions, cumulative race appearances robustly predict survival; however, there is no additional predicted effect of appearances under conditions of higher uncertainty.

TABLE 6 Additional tests of broader interpretations of the theory

Variable	6–1	6–2	6–3	6–4	6–5	6–6
Gasoline technology	1.36 [1.02,1.83]	1.36 [1.02,1.83]	1.36 [1.01,1.82]	1.37 [1.02,1.83]	1.37 [1.02,1.83]	1.37 [1.02,1.84]
Startup	1.52 [1.17,1.96]		1.60 [1.25,2.03]	1.61 [1.26,2.05]	1.60 [1.25,2.04]	1.46 [1.13,1.89]
Ln(appearances+1)	0.51 [0.38,0.67]	0.58 [0.48,0.69]	0.59 [0.50,0.70]	0.57 [0.48,0.68]	0.57 [0.48,0.67]	0.65 [0.54,0.78]
Startup X ln(appearances+1)	1.20 [0.86,1.68]					
Young		1.26 [1.01,1.57]				
Young X Ln(appearances+1)		0.89 [0.58,1.38]				
Pre-1907			1.03 [0.84,1.25]			
Pre-1907 X Ln(appearances+1)			0.73 [0.48,1.12]			
Spinoff				0.74 [0.46,1.17]		0.79 [0.49,1.28]
Spinoff X Ln(appearances+1)				1.05 [0.67,1.62]		
Top-ten parent spinoff					0.50 [0.16,1.54]	0.85 [0.31,2.31]
Top-ten parent spinoff X Ln(appearances+1)					1.32 [0.75,2.33]	
Total production (t − 1)						0.92 [0.86,0.98]
MI						0.93 [0.67,1.29]
NY						1.10 [0.83,1.45]
OH						0.98 [0.72,1.32]
IN						0.59 [0.39,0.89]
Produced models						1.06 [0.88,1.28]
Cohort 1 (pre-1905)						1.00 [1.00,1.00]
Cohort 2 (1905–1909)						1.04 [0.79,1.37]
Cohort 3						1.00 [1.00,1.00]
Year fixed effects	Included	Included	Not included	Included	Included	Included
Observations	2120	2120	2120	2120	2120	2120

Note: Exponential failure hazard models predicting firm exit. Hazard ratios reported. 95% confidence intervals in parentheses.

1.7.6 | Interpretation of the findings

This quasi-replication led us to the following conclusions: The data do not allow us to distinguish whether winning, placing second or third, participating, or some combination of these outcomes is associated with survival. Each is too highly correlated with the others to reliably isolate the relative effects. That is, the data do not allow us to adjudicate between the general loose-coupling argument and the narrower rank-ordering argument proposed in the original study. We also fail to find support for the idea that this affiliation is more beneficial under conditions of higher uncertainty. Thus, it is helpful to consider whether races should have been expected to legitimize individual firms in the first place. As we collected data on car races, we noticed that the purpose of these races evolved from contestations around what constituted quality due to the absence of a dominant design, to assessments of quality of the participating vehicles, to providing entertainment and determining the best drivers, rather than the best manufacturers. As racing changed from a test-bed to its own entertainment industry organized around speed races, the interpretation of entry into a race as a strategic choice becomes less clear.

Rao details how contests may have functioned to legitimize the industry, and quotes Frank Munsey as stating in 1906 that “the uncertain period of the automobile is now past” (quoted in Rao, 1994: 35). However, the original study also points out that third party organizations had strong incentives to organize races, even if they did not charge admission to the spectacles (Rao, 1994: 35). That is, very early in the industry's history, racing developed into an industry in and of itself. As Henry Ford wrote in January of 1902, well before he dominated the industry, “There is a barrel of money in this [racing] business ... I expect to make $ when I can't make ¢ at manufacturing” (Ford, 1902). Thus, as races evolved into an entertainment industry early on, their legitimizing power may have diminished.

The historical record suggests that at least some race appearances were not the automobile firms' strategic choice, but instead that of race car drivers or their sponsors. For instance, one of the most famous race car drivers from the period, Barney Oldfield, had achieved celebrity status—to the point of being able to charge $4000 for a personal appearance in the 1910s—for his ability to break speed records (Median income in 1905 was $10/week).[15] Oldfield had become a brand of his own and hence participated in races and contested in cars based on winning potential—not based on manufacturer demands. Our analysis indicates that between 1902 and 1912 Oldfield raced cars made by 25 different manufacturers. Additional historic evidence suggests that Oldfield bought vehicles and modified them to win the races that he entered (e.g., he used Ford to win the Manufacturer's Challenge Cup in 1902 and Benz to set a variety of speed records at Daytona Beach in 1910) (International Motorsport Hall of Fame). Furthermore, Oldfield often used vehicles by different manufacturers for races of different lengths within the same event (e.g., Cino for the five-mile race, Benz for the one-mile race, and Christie for the two-mile race at the Cleveland automobile club races in 1912). Therefore, if sponsors and drivers were choosing the cars they believed most likely to bring them an advantage in a race, then they would choose the best cars. If this is the case, then both race participation and consequent survival are *outcomes* of firm quality.

It is also likely that firms fundamentally differed in their ability to operate factories and make strategic decisions. In 1906 Mr. Flanders was able to increase the production at the Ford Factory from 20 to 150 cars a day merely by rearranging the existing equipment in the entire factory (Glasscock, 1937: 118). Similarly, through improved tools, Cadillac was able to reduce the time required for a particular process by one-tenth in 1905 (The Detroit News, June 17, 1923). From a marketing perspective, Maxwell opened up a new market segment for itself and achieved tremendous selling to the farming community by adopting the novel strategy of advertising in farm weeklies and rural newspapers (Berger, 1979: 36). Similarly, entrepreneurs, who sensed the mass market potential of the automobile, focused on designing utility-oriented quality vehicles at lower cost rather than catering to the existing market for high-cost vehicles (Flink, 1988: 33).

[15]United States. 1908. *Census of Manufactures: 1905. Earnings of Wage-Earners.* Washington, DC: Government Printing Office. Page 11, Table 2. https://hdl.handle.net/2027/nnc1.cu56779232?urlappend=%3Bseq=15

Regardless of who decided to enter cars into races, firms or drivers, the interpretation of the results as consistent with the legitimation mechanisms relies on there not being an omitted variable that also drives survival. If race car drivers chose the best cars for races and better cars implied greater chances of survival or if the most able managers whose firms had better survival chances chose to enter their cars in races, then an omitted variable problem remains. Interpreting the regression coefficients as causal requires assuming that we have perfectly controlled for the impact of automobile quality and firm capability.

2 | DISCUSSION AND CONCLUSION

We have conducted a narrow replication and have successfully replicated the results from Rao's 1994 paper after reconstructing the dataset from the original data sources. In the original study, an association between victories in auto races and firm survival is shown, and the reputation and legitimation mechanism is theorized. Our findings are consistent with those reported in the original study: cumulative victories in certification contests are associated with lower exit rates and these effects are not stronger for startup firms.

We then extended our analysis and conducted a quasi-replication. Following the prediction of the original paper that reputational benefits from race victories should accrue more to firms under conditions of higher uncertainty, such as startups, we looked for differential effects of races during early industry stages and recent races. We found no evidence of such differential effects. We also collected data on placing second and third and appearing in races. These three variables separately and together also negatively predict firm exit and there is not enough information in the data to separate their effects from those of victories, as they all are very strongly correlated. The result is not consistent with idea that rank-order in certification contests affects firm survival; however, it is consistent with the idea of loose coupling of symbols, whereby reputational benefits accrue to those who are affiliated with certification contests.

Interpreting the results as evidence that firms survived *because* they were legitimized by and gained reputational advantages from participation in certification contests requires additional assumptions. We must assume that racing is not a proxy for firm quality or strategic ability. To investigate this concern, we controlled for firm-level characteristics that may simultaneously predict contest victories and survival: firm size, whether the firm was a spinoff or a spinoff of a high-quality incumbent, firm location, and lagged number of produced models. The predicted effect of certification contest victories is robust to the inclusion of any or all of these variables. However, the theory also predicts that certification should matter more under high uncertainty, yet neither we nor the original author have been able to find support of this premise in the context of racing and the automobile industry. Importantly, we are unable to develop fine measures of the quality of entrepreneurial decision making, or manufacturing or marketing capabilities. Yet, such differences are the subject of several historical accounts of the period and contemporary observers.

We caution that the only way to interpret the results of our replication study as demonstrative of a causal effect of race participation on legitimation and hence survival is to assume that entry into a race is conditionally uncorrelated with underlying firm capabilities or product quality that also predict survival. That is, the quality measures offered in the quasi-replication must completely control for quality. For example, it cannot be that race entry is a sign of competent managers who are more likely to help their firms survive longer over and above spinoff status, parent top ten status, or competence measured by lagged production. If it is the case that entries are determined by drivers or racing teams, it cannot be that these choices measure firm quality aspects not picked up by the offered controls.

That is, entry into races must be a (conditional) random event.[16] If we are uncomfortable with this assumption, we should not necessarily conclude that there is no causal effect either. For example, despite our inability to find a broad statistical pattern, Henry Ford believed that winning races had a legitimating, or at least attention grabbing, effect:

> *“That 'Model B'—the first four-cylinder car for general road use—had to be advertised. Winning a race or making a record was then the best kind of advertising. … [Model B won the race] making a record that went all over the world! That put "Model B" on the map”* (Ford, 2003 [1922]).

However, as revealed in the continuation of Ford's quote, he also believed that at the end of the day, competence and quality dominated:

> *“—but not enough to overcome the price advances. No stunt and no advertising will sell an article for any length of time. Business is not a game”* (Ford, 2003 [1922]).

Peculiarities of this study's setting may make its findings generalizable to some, but not all, contexts. Whereas in the American automobile industry audiences could observe the quality of cars that appeared in certification contests (e.g., their speed and propensity to break down), quality of products in other industries may be less easily determined. Specifically, in industries where quality is determined in a more subjective way (e.g., fashion, wine, art), victories in certification contests may be more predictive of performance outcomes. We suspect that placing first in the rankings of industries where quality determination contains a substantial subjective component the theoretical mechanisms explicated in Rao's 1994 study may be more pronounced. We thus encourage future studies to examine the differences in the role of certification contests in industries with objective versus subjective determinations of quality (e.g., Waguespack & Salomon, 2015).

Finally, we emphasize that our findings do not negate the theory presented in the original paper. As Bettis, Helfat, and Shaver (2016) point out, “one study that fails to replicate the previous result does not mean that the previous result has been rejected. It does mean that the balance of evidence regarding the existing results moves toward questioning the original result.”

We expect that future studies on the role of certification contests for firm performance and survival will be conducted in other settings, so we can properly ascertain the boundary conditions of these theories.

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[16]Consistent with these ideas, our initial analysis of management studies that consider the role of certification contests revealed a lack of conclusive empirical evidence that winning such contests is better than losing. For example, firms with the “best” as well as those with “worst” boards of directors, as ranked by *Business Week*, enjoy positive abnormal returns (Johnson et al., 2005). Other studies have also failed to find evidence that winning certification contests is beneficial (e.g., Fulmer et al., 2003). Graffin et al., 2008) suggest that more research is needed to understand the circumstances under which winning certification contests has a positive effect on firm performance.

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RESOURCES

This article has earned an Open Data badge for making publicly available the digitally-shareable data necessary to reproduce the reported results. The data is available at five.dartmouth.edu. Learn more about the Open Practices badges from the Center for Open Science: https://osf.io/tvyxz/wiki.

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