The Effect of Contract Regulation on Franchising

by

Jonathan Klick, Bruce H. Kobayashi, and Larry E. Ribstein*

Previous work on the regulation of termination clauses in franchise contracts has ignored the ability of parties to contract around state law. Using data on two national fast-food restaurants, we find that Washington, D.C.'s termination restriction which did not restrict choice-of-law provisions had little systematic effect on business practice, while Iowa's termination restriction which did prohibit choice-of-law provisions had a large negative effect on franchising. (JEL: D86, K12, K20, L14)

1 Introduction

Franchising is an important and frequently studied form of organization. Prior articles have used the franchising form to examine the general nature of intra- versus interfirm contracting[1] and to analyze how contracts and incentives are used to reduce transaction and agency costs. Paul Rubin (1978) applied the insights of transaction cost economics to explain the existence of franchising as a business form. Rubin argued that existing explanations for use of the franchise form based on capital constraints were implausible, and that insights from Coase's (1937) theory of the firm better explain the existence of the franchise form. In addition, studies of franchise regulation illustrate how the regulation of the contractual relationship between franchisors and franchisees affects contracting and the organization of firms.[2]

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* Jonathan Klick (corresponding author), University of Pennsylvania Law School; Bruce Kobayashi, George Mason University School of Law; and Larry Ribstein, University of Illinois College of Law. We thank Ted Eisenberg and Andreas Roider as well as the other participants at the 2011 *JITE* conference on "Testing Contracts."

[1] See Klein (1995) (discussing the economics of franchise contracts); Lafontaine and Masten (1995) (discussing literature generally); Brickley, Misra, and Van Horn (2006) (finding contract duration in franchise contracts is positively related to franchisee's level of specific investments), Brickley (1999) (examining incidence of specific contractual provisions in franchise contracts).

[2] See, e.g., Brickley, Dark, and Weisbach (1991), Muris and Beales (1994) (examining state regulation of termination); Marvel (1995) (examining FTC regulation of gasoline franchising); Smith (1982) (examining state regulation of automobile dealers).

The use of termination clauses has been central in much of this literature. Rubin (1978) and Klein (1995) both argue that termination clauses serve as part of a bond whereby a franchisee risks losing his rents if he engages in ex post opportunism. Brickley, Dark, and Weisbach (1991) test this idea using state laws that restrict termination clauses, generally finding support for the bonding hypothesis. However, this study relies on cross-sectional comparisons from which it is difficult to make causal inferences. Further, as we suggested in Klick, Kobayashi, and Ribstein (2009), it is surprising that these laws, on their own, have any effect on franchise activity given the ability of parties to contract around termination restrictions in many cases.

In this article, we address both of these issues using some limited data on franchising in the fast-food industry. Using panel data, we examine the effects of the termination restriction law passed in Iowa in 1992 and the removal of a similar law in the District of Columbia in 1998. Panel data provide the opportunity to parse out idiosyncratic differences across states in isolating the effect of a given law in ways not possible in purely cross-sectional data. Also, these two particular laws are interesting in that the Iowa law severely restricted the ability of the parties to contract around the termination limitation, while the D.C. law, when it was in effect, did not foreclose the possibility of waiver or choice-of-law-and-forum clauses.

Our examination of the effects of these law changes suggests that the Iowa law did affect the degree of franchise activity that occurred in Iowa. Given the high transaction costs created by restrictions on clauses that would undo the termination law, the termination restriction appears to have reduced franchise activity. The D.C. termination law, on the other hand, which had no such restrictions, had ambiguous effects on franchising in the jurisdiction. This leaves open the possibility that parties simply contracted around the law, as would be suggested by the Coase theorem.

On a methodological note, because our analysis focuses on very limited variation in the legal variable that provides our statistical identification, we explore a number of new methods for making statistical inferences. While these tools have yet to have much of an impact in the empirical law and economics literature, they deserve attention since the problems we face are quite common when investigating the effects of legal changes.

In section 2, we briefly review the economic theory regarding the use of termination clauses in franchise contract and we describe the existing empirical evidence regarding their effects. In section 3, we describe the panel data we collected. Section 4 presents our empirical results, while section 5 discusses the problem of statistical inference in these settings. We conclude in section 6.

2   Termination Clauses in Franchise Contracts

As laid out in Rubin (1978) and Klein (1995), franchise contracts give rise to problems of opportunism. Poor performance on the part of a given franchisee generates a negative externality borne by the franchisor and other franchisees. This

negative externality degrades the value of the trademark. Ex ante, franchisees would like to commit to a given level of performance, but this commitment is not credible. In order to make the commitment credible, the franchisee posts a bond through the payment of a franchise fee and makes other sunk investments, while agreeing to broad termination rights for the franchisor. In expectation, these expenditures will be recouped through a flow of future rents.

As made clear in Klein (1995) as long as the net present discounted value of these future rents exceeds the gain available from cheating (i.e., poor performance), the franchisee will not shirk under these conditions, assuming the franchisor detects cheating perfectly. In the case where detection is imperfect, the size of the bond and sunk investments can be scaled to offset the detection problem.

However, these broad termination rights may create an opportunism problem on the part of the franchisor. Specifically, the franchisor can engage in cream skimming in which it terminates contracts when a given franchise proves to be unexpectedly profitable for the franchisee. In such a scenario, the franchisor can resell the unit at terms more favorable to itself or run the unit directly. While reputational concerns may constrain this franchisor opportunism, if franchisees are poorly informed or make systematic mistakes, termination clauses may trade one form of opportunism for another.

This cream-skimming concern has motivated a number of jurisdictions to restrict such clauses, requiring a showing of good cause for termination on the part of franchisors. If the termination clauses serve to reign in franchisee opportunism, limiting termination to good cause is likely to leave some opportunism unchecked in a world of incomplete contracts, litigation costs, and court error. On the margin, some franchise units will become economically unprofitable, leading the franchisor to reduce its activity, reducing social welfare in the process.[3] If the cream-skimming concern is dominant, however, these laws could improve social welfare by constraining the franchisor's opportunism.

If these laws primarily serve to limit franchisor opportunism, the effect on franchising activity is ambiguous. If franchisees no longer fear ex post opportunism on the part of franchisors, they could be willing to open more franchises at a given set of terms. However, if the opportunism took the form of tricking franchisees into opening many poor locations in the hope of discovering a few unexpected prizes, termination restrictions could lead to a reduction of franchising.

To test these competing hypotheses, Brickley, Dark, and Weisbach (1991) compare the extent of franchising in states that restrict termination and those that do not. They find a lower share of franchising in restricted states than in non-restricted states using cross-sectional data. To address the efficiency question, they separately analyze industries serving primarily non-repeat customers (where franchisee

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[3] Some units might be run by the franchisor at a lower, but still positive, profit. However, as in the Klein model, some units that cannot be run profitably by the franchisor due to its inability to control agency costs could be run profitably by a well incentivized franchisee whose opportunism is constrained. Once this constraint is reduced, the unit becomes unprofitably regardless of which entity manages it.

opportunism is more likely to be a problem), finding the effect to be more pronounced in these industries. This suggests that franchisee opportunism concerns dominate concerns over cream skimming, and the termination restrictions affect organizational form. The restrictions then presumably have a negative effect on economic efficiency. They find complementary evidence using cross-sectional firm-level data.

Cross-sectional studies, however, have significant limitations due to omitted variable biases. Specifically, law passage may be endogenous to other factors that affect franchising patterns. Modern panel data techniques, specifically differences-in-differences studies using fixed effects, can mitigate these concerns. If the portion of the omitted variables that is the source of the bias is time-invariant, this kind of research design can yield causal inferences. Brickley, Dark, and Weisbach (1991) could not implement such a design because of data limitations. Even if the omitted variable bias is generated by changing unobservable characteristics of a state, if those changes are linear, the bias can be controlled through the inclusion of state-specific trends.

Another concern with the termination law analysis involves the ability of parties to contract around such laws. Brickley (2002) suggests that some contractual changes do occur in the face of these laws, finding that franchisors located in states with restrictions tend to use lower front-end fees and higher royalties, presumably spending more to monitor franchisee behavior. Again, these results are based only on cross-sectional data, so causal inference is problematic.

However, even if the effects are causal, they are presumably not the parties' first response to termination restrictions. Namely, as discussed above and in Klick, Kobayashi, and Ribstein (2009), in some states it would be simple to contractually avoid the effect of the statutory termination restrictions. Although states generally do not permit direct waiver of these restrictions, which are intended for the protection of one of the contracting parties, parties can achieve a similar effect through a clause providing that the contract is to be interpreted and enforced under the law of a state that does not regulate franchise termination. Under applicable choice-of-law rules, whether a court will enforce a choice-of-law clause depends on the contacts between the parties and transaction on the one hand and the chosen jurisdiction on the other, whether a state with closer contacts seeks to regulate the transaction, and on the nature of this regulation (O'Hara and Ribstein, 2009, ch. 3). Because courts have significant discretion under these rules whether to enforce another state's restrictions, the parties might maximize the chance that the clause will be enforced by adding clauses to the agreement providing that the dispute will be decided by a court that is likely to enforce the parties' choice-of-law clause, and that the parties consent to jurisdiction in the designated forum.

Enforceability of such choice-of-forum and jurisdiction clauses has been supported by the U.S. Supreme Court.[4] Indeed, the Court's 1985 ruling enforcing

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[4] *M/S Bremen v. Zapata Off-Shore Co.*, 407 U.S. 1 (1972); *Carnival Cruise Lines, Inc. v. Shute*, 499 U.S. 585 (1991).

a consent-to-jurisdiction clause in a franchise contract[5] provided an important impetus to the enforcement of choice-of-forum and choice-of-law clauses in these contracts. U.S. federal courts quickly followed with cases enforcing choice-of-law clauses in these contracts.[6] States could avoid the effect of these rulings by enacting statutory provisions explicitly voiding choice-of-law and choice-of-forum clauses. However, only two states, Washington and Iowa, void both types of provisions in their franchise protection statutes.[7] Parties could avoid regulation of franchises located in other states by choosing the law of a non-regulating state and a forum that will enforce the contract's choice-of-forum clause. Even if the *franchisor* is located in a state that does not enforce choice-of-law or choice-of-forum clauses, the franchisor can avoid the effect of franchise regulation of *franchises* located in states that do enforce the clauses (Klick, Kobayashi, and Ribstein, 2009).

Use of choice-of-law-and-forum clauses is distinct from other types of contractual or organizational responses in that it does not require the firm to make costly tradeoffs. The effect of choice-of-law-and-forum clauses stems from inherent limits on states' regulatory powers in a federal system, and can produce results closer to pure regulatory arbitrage than the courts in the regulating state are likely to permit under that state's law (Kobayashi and Ribstein, 1999). The prior economics literature had completely ignored this availability of regulatory arbitrage.

Given the parties' ability to contract around termination restrictions using choice-of-law and choice-of-forum clauses, it would be surprising if such laws, on their own, would have any effect on franchise activity. Franchise contracts routinely contain contractual choice-of-law-and-forum clauses, and the courts have allowed such choices when not explicitly precluded by a state (Kobayashi and Ribstein, 1999; Klick, Kobayashi, and Ribstein, 2009). Our earlier empirical work in this area suggests the effectiveness of contractual choice of jurisdiction as a mechanism for regulatory arbitrage. Taking into account state variation in restricting choice-of-law-and-forum clauses, we found that termination restrictions only have an effect if the states' statutes explicitly restrict use of contractual choice-of-law-and-forum clauses (Klick, Kobayashi, and Ribstein, 2009).

Our previous work in this area uses panel data methods to mitigate the endogeneity concerns of the existing economics literature and it does allow for a more complete consideration of the heterogeneity in the strictness of the state termination restrictions. However, it relies on a fairly blunt metric of franchise activity, namely employment in industries with a high degree of franchising in them. In this article, we instead rely on firm-level franchising data.

To address the issue of legal heterogeneity, we focus on two very different termination restrictions. First we examine the termination restriction introduced in Iowa in

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[5]   *Burger King v. Rudzewicz*, 471 U.S. 462 (1985).

[6]   *Tele-Save Merchandising v. Consumers Distributing*, 814 F.2d 1120 (6th Cir. 1987); *Modern Computer Systems, Inc. v. Modern Banking Systems, Inc*., 871 F.2d 734 (8th Cir. 1989).

[7]   For a comprehensive listing of State Franchise Protection Statutes, see Klick, Kobayashi, and Ribstein (2009, Table 1).

1992 which also included restrictions on waiver, choice-of-law, and choice-of-forum clauses. These additional restrictions make it very difficult to avoid the termination restriction through contractual mechanisms, presumably leading to a relatively large change in franchising behavior. We also examine the federal repeal of Washington, D.C.'s termination restriction in 1998. The D.C. law was not one of the few jurisdictions restricting parties' ability to avoid regulation through waiver, choice-of-law, or choice-of-forum clauses (see Klick, Kobayashi, and Ribstein, 2009, Table 1).[8] Presumably then, if termination clauses serve some economic purpose, the parties could have easily contracted around the restriction through one of these other avenues, suggesting that any change in franchising behavior occurring after repeal would likely be small.

Unfortunately, we have no way of testing the efficiency effects of any of these laws, lacking a comparison between repeat and non-repeat businesses. Instead, we merely aim to provide a better design for causal inference and to highlight the importance of more careful consideration of legal subtleties.

3   Franchising Data

The data used by Brickley, Dark, and Weisbach (1991) came from the U.S. Department of Commerce publication "Franchising in the Economy." These data were not published beyond 1986, requiring us to find a different data source for our analysis.

We examined the Uniform Franchise Offering Circular (UFOC) filed by a number of fast-food franchisors with the Maryland Attorney General's Office. The UFOC (since renamed the Franchise Disclosure Document), which is a required filing in 15 states contains an item (item 20) that discloses the number of outlets franchised and franchisor operated in each state. Unfortunately, for our purposes, this item was only required beginning in 1992, although some franchisors began disclosing this information in the preceding years.

Of the large fast-food franchisors, the only ones that included the information for item 20 before 1992 were Burger King (disclosure beginning in 1990) and Domino's Pizza (disclosure beginning in 1989). We focus much of our analysis on those two franchisors. Because of its importance in the industry, we also include McDonald's

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[8] Both the Iowa and D.C. statutes required notice and good cause for a franchise to terminate its contract with a franchisee. Both statutes also required cause for non-renewal, and allowed a franchisee a right to cure defects. Thus, with respect to the termination/renewal provisions, both the Iowa and D.C. statutes were substantively similar. The Iowa statute did include other substantive provisions not included in the D.C. statute, including franchisor liability for encroachment and a provision that allowed a franchisor to purchase supplies, equipment, services, etc. from sources of the franchisee's choosing. The encroachment provisions originally enacted in the 1992 Iowa statute were weakened in a 1995 amendment to the statute. See Iowa Code § 523H (1992, 1995).

Table 1
Number of Franchised Units per State

Firm	Mean	St. dev.	IA 1992 count	DC 1998 count
Burger King	120	128	47	9
Domino's Pizza	73	73	17	13
McDonald's	188	188	88	27

Note: Data from UFOC item 20 filed with Maryland Attorney General's Office. Sample covers 1990–2000 for Burger King, 1989–2000 for Domino's Pizza, and 1992–2000 for McDonald's.

(disclosure beginning in 1992) in our analysis of the D.C. termination restriction.[9] Descriptive statistics for these firms are presented in Table 1.

4 Contract Regulations (Sometimes) Affect Behavior

Our research design examines the number of franchised units in a given state during a given year, controlling for deflated state per capita income, as well as state- and year-fixed effects. We did examine other control variables, primarily demographic controls, we chose to omit them in the results presented because (1) they did not affect the results of interest and (2) because demographic data are linearly interpolated between census years, inclusion of these data in the regressions is unlikely to be informative beyond the specifications that also include linear state-specific trends.

Because of concerns regarding scale differences across states, we present regression specifications that use both the level and the natural log of the dependent variable. Our regressor of interest, the termination restriction, is an indicator taking the value of 1 if the state restricted the application of termination clauses in franchise contracts and 0 otherwise. To account for dependence in our data, we cluster standard errors at the state level. In our first set of results, we pool the effect of the Iowa and the Washington, D.C. restrictions in Table 2.

In addition to a general lack of statistical significance, it is apparent that there is no robust effect of the laws across the two firms we examined. However, given the discussion above about the importance of considering how firms might be able to contract around laws such as these, we proceed to examine the two laws separately.

First we examine the highly restrictive Iowa law which prohibits the application of not only termination clauses, but it also explicitly prohibits the application of waiver, choice-of-law, and choice-of-forum clauses. We present these results in Table 3.[10]

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[9] We examined a number of other franchisors in the context of the D.C. law, finding qualitatively similar results to those that follow.

[10] Note that the Table 3 and Table 4 regressions omit observations from Washington, D.C. We do this to avoid the inclusion of any effects arising from the D.C. termination restriction that will be examined later.

Table 2
Effect of Termination Restrictions on Franchised Restaurants

| | Burger King | | Domino's Pizza | |
	Franchise units	ln(franchise units)	Franchise units	ln(franchise units)
Termination restriction	8.38 (19.53)	−0.08*** (0.02)	−0.11 (5.17)	−0.11 (0.21)
State effects	yes	yes	yes	yes
Year effects	yes	yes	yes	yes
State trends	no	no	no	no

Notes: *** $p < 0.01$ (against two-sided test of zero effect); ** $p < 0.05$ (against two-sided test of zero effect); * $p < 0.10$ (against two-sided test of zero effect); standard errors in parentheses clustered at state level. Sample includes the 50 states and the District of Columbia for the period 1990–2000 ($n = 561$) for Burger King and 1989–2000 for Domino's Pizza ($n = 612$). Regressions control for constant state per capita income. Franchise data come from UFOC's (item 20) filed with Maryland State Attorney General's Office.

Table 3
Effect of 1992 Iowa Termination Restriction on Franchised Restaurants

| | Burger King | | Domino's Pizza | |
	Franchise units	ln(franchise units)	Franchise units	ln(franchise units)
Termination restriction	−22.59 (6.30)***	−0.06 (0.04)	−7.14 (3.50)**	−0.40 (0.04)***
State effects	yes	yes	yes	yes
Year effects	yes	yes	yes	yes
State trends	no	no	no	no

Notes: *** $p < 0.01$ (against two-sided test of zero effect); ** $p < 0.05$ (against two-sided test of zero effect); * $p < 0.10$ (against two-sided test of zero effect); standard errors in parentheses clustered at state level. Sample includes the 50 states for the period 1990–2000 ($n = 550$) for Burger King and 1989–2000 for Domino's Pizza ($n = 600$). Regressions control for constant state per capita income. Franchise data come from UFOC's (item 20) filed with Maryland State Attorney General's Office.

We find that the effect of the Iowa law is uniformly negative and the effect is generally statistically significant for both firms in both the log and the level specifications. The point estimates suggest that the Iowa restriction was associated with a decline in Burger King franchises, relative to the Iowa baseline and relative to the amount of franchising occurring elsewhere, of about 46%. For Domino's Pizza, the comparable magnitude is 33%.

While the fixed-effects model accounts for constant unobserved heterogeneity across states that could lead to omitted variables bias in pure cross-sectional com-

Table 4
Effect of 1992 Iowa Termination Restriction on Franchised Restaurants:
Allowing for State Trends

| | Burger King | | Domino's Pizza | |
	Franchise units	ln(franchise units)	Franchise units	ln(franchise units)
Termination	−8.31	−0.13	−15.70	−0.92
restriction	(1.42)***	(0.04)***	(2.53)***	(0.05)***
State effects	yes	yes	yes	yes
Year effects	yes	yes	yes	yes
State trends	yes	yes	yes	yes

Notes: *** $p < 0.01$ (against two-sided test of zero effect); ** $p < 0.05$ (against two-sided test of zero effect); * $p < 0.10$ (against two-sided test of zero effect); standard errors in parentheses clustered at state level. Sample includes the 50 states for the period 1990–2000 ($n = 550$) for Burger King and 1989–2000 for Domino's Pizza ($n = 600$). Regressions control for constant state per capita income. Franchise data come from UFOC's (item 20) filed with Maryland State Attorney General's Office.

parisons, it does not account for changing heterogeneity that could be associated both with law adoption and franchising. To attempt to mitigate this concern, we present regressions that include linear state-specific trends in Table 4.

Once again, we find uniformly negative and statistically significant effects associated with the Iowa termination restriction, relative to Iowa's baseline and relative to developments in franchising elsewhere. As for the endogeneity concerns, the trend specifications suggest, if anything, that the earlier results may have actually been understated.

Moving on to the Washington, D.C. termination restriction, which was repealed by the U.S. Congress in 1998, we note that compared to the Iowa law, the D.C. law is relatively easy to contract around since it contains no explicit limitation on waiver, choice-of-law, or choice-of-forum clauses. While it is possible that a court could read such limitations into any transaction, especially with respect to waiver clauses, it is by no means guaranteed.[11] Because of this, we predict that any effect of the law is likely to be smaller and less precise than that associated with the Iowa law. We present the first set of D.C. results in Table 5.

In one case the effect of the D.C. law is of a different sign than that associated with the Iowa law passage. While this may be suggestive that the D.C. law is different in unexpected ways than the Iowa law, the sign difference within the Burger King specifications could be suggestive of a general lack of robustness. We see more evidence of this when we examine regressions that include state-specific linear trends in Table 6.

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[11] For a discussion of these issues, see Klick, Kobayashi, and Ribstein (2009).

Table 5
Effect of 1998 D.C. Termination Restriction Repeal on Franchised Restaurants

| | Burger King | | Domino's Pizza | |
	Franchise units	ln(franchise units)	Franchise units	ln(franchise units)
Termination restriction	31.41 (5.36)***	−0.11 (0.03)***	6.77 (2.93)**	0.17 (0.03)***
State effects	yes	yes	yes	yes
Year effects	yes	yes	yes	yes
State trends	no	no	no	no

Notes: *** $p < 0.01$ (against two-sided test of zero effect); ** $p < 0.05$ (against two-sided test of zero effect); * $p < 0.10$ (against two-sided test of zero effect); standard errors in parentheses clustered at state level. Sample includes 49 states (Iowa is omitted) and the District of Columbia for the period 1990–2000 ($n = 550$) for Burger King and 1989–2000 for Domino's Pizza ($n = 600$). Regressions control for constant state per capita income. Franchise data come from UFOC's (item 20) filed with Maryland State Attorney General's Office.

Table 6
Effect of 1998 D.C. Termination Restriction Repeal on Franchised Restaurants:
Allowing for State Trends

| | Burger King | | Domino's Pizza | |
	Franchise units	ln(franchise units)	Franchise units	ln(franchise units)
Termination restriction	−3.51 (1.70)**	−0.54 (0.03)***	4.32 (1.79)**	−0.05 (0.05)
State effects	yes	yes	yes	yes
Year effects	yes	yes	yes	yes
State trends	yes	yes	yes	yes

Notes: *** $p < 0.01$ (against two-sided test of zero effect); ** $p < 0.05$ (against two-sided test of zero effect); * $p < 0.10$ (against two-sided test of zero effect); standard errors in parentheses clustered at state level. Sample includes 49 states (Iowa is omitted) and the District of Columbia for the period 1990–2000 ($n = 550$) for Burger King and 1989–2000 for Domino's Pizza ($n = 600$). Regressions control for constant state per capita income. Franchise data come from UFOC's (item 20) filed with Maryland State Attorney General's Office.

What is most evident is the lack of stability in the parameter estimates. Between Tables 5 and 6, we see large changes in the magnitude of the coefficients as well as sign changes. Within each table as well, there are indications of fragility of the estimates. We take this as weak evidence of the lesser effect of the D.C. law, though,

of course, it could also be a sign of model misspecification or other empirical problems.[12]

We see a similar lack of robustness if we examine the effect of the D.C. law's repeal on the franchising activities of McDonald's in Table 7.

Table 7
Effect of 1998 D.C. Termination Restriction Repeal on Franchised
McDonald's Restaurants

| | McDonald's | | | |
	Franchise units	ln(franchise units)	Franchise units	ln(franchise units)
Termination	26.94	0.07	−20.09	−0.22
restriction	(5.30)***	(0.02)***	(4.20)***	(0.02)***
State effects	yes	yes	yes	yes
Year effects	yes	yes	yes	yes
State trends	no	no	yes	yes

Notes: *** $p < 0.01$ (against two-sided test of zero effect); ** $p < 0.05$ (against two-sided test of zero effect); * $p < 0.10$ (against two-sided test of zero effect); standard errors in parentheses clustered at state level. Sample includes 49 states (Iowa is omitted) and the District of Columbia for the period 1992–2000 ($n = 450$). Regressions control for constant state per capita income. Franchise data come from UFOC's (item 20) filed with Maryland State Attorney General's Office.

From these analyses, the best estimate is that the Iowa termination restriction had a significantly negative, in terms of both statistical significance and economic importance, effect on franchising, while the weaker D.C. law did not have a clear effect. This is consistent with the notion that contractual regulations only bite when it is difficult to contract around them. This point may seem trivial, but it is one that has been missed in the voluminous economics literature on franchising.

While we believe we have made methodological and substantive improvements in the analysis in this field, our data do not allow us to investigate the economic efficiency of termination clauses or laws that disallow them. The laws do seem to affect economic behavior when they increase the costs of contracting around them.

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[12] As Roider (2012) makes clear, failure to account for trends can be a source of misspecification. Our primary concern here is the lack of a uniform effect of the law in terms of sign both across firms and across specifications. This kind of fragility suggests that we cannot make an inference about the effect of the law with any degree of confidence. Eisenberg (2012) suggests, based on graphical analyses, that the problem may be one of a failure to find decent counterfactuals in the available data.

5 Problems of Inference

Drawing inferences about a single law change is complicated for both intuitive and more technical reasons. One intuitive concern involves the choice of counterfactuals against which to judge the effect of the Iowa law. In a sense, the preceding analysis compares the experience of Iowa to the pooled experience of the rest of the U.S. This may be problematic in that Iowa might simply have been following regional trends that are obscured when compared to the rest of the country.

To mitigate this concern, we performed the same analysis restricting attention to the Midwestern states in Table 8. For brevity, we present only the log specifications.

Table 8
Effect of 1992 Iowa Termination Restriction on Franchised Restaurants: Midwest

	Burger King		Domino's Pizza	
	ln(franchise units)	ln(franchise units)	ln(franchise units)	ln(franchise units)
Termination	−0.12	−0.17	−0.24	−0.80
restriction	(0.05)*	(0.04)***	(0.05)***	(0.05)***
State effects	yes	yes	yes	yes
Year effects	yes	yes	yes	yes
State trends	no	yes	no	yes

Notes: *** $p < 0.01$ (against two-sided test of zero effect); ** $p < 0.05$ (against two-sided test of zero effect); * $p < 0.10$ (against two-sided test of zero effect); standard errors in parentheses clustered at state level. Sample includes the 12 Midwestern states for the period 1990–2000 ($n = 132$) for Burger King and 1989–2000 for Domino's Pizza ($n = 144$). Regressions control for constant state per capita income. Franchise data come from UFOC's (item 20) filed with Maryland State Attorney General's Office.

Examining just the Midwest, we find no evidence that the earlier analysis was misleading. In this restricted sample, we find uniformly negative effects of the Iowa law, and the magnitude of the effect is comparable to that found earlier. Also, despite the large decline in sample size, we continue to find statistically significant effects.

Another worry that arises is one of dependence. Applied empirical economists have recognized the concerns about dependence at least since Bertrand, Duflo, and Mullainathan (2004) which suggested clustering standard errors at the state level in policy analyses such as this one. The concern there is that many economic series exhibit serial correlation and laws themselves are sticky rather than randomly implemented or repealed each period. In such applications, standard errors can be severely understated when there is positive dependence (or overstated when there is negative dependence). Clustering accounts for this dependence in certain circumstances by inflating or deflating the standard error estimates accordingly.

Rather less attention has been paid to within period dependence. That is, in the current context, if Burger King has some national strategy that affects its activities in all states but is not sufficiently accounted for by the control variables (specifically the year-fixed effects) state observations within a given year will not be conditionally independent. This could lead to problems of inference due to incorrectly calculated standard errors. Cameron, Gelbach, and Miller (2011) provide a procedure to account for this multi-way clustering. We present our estimates with these standard errors in Table 9.

Table 9
Effect of 1992 Iowa Termination Restriction on Franchised Restaurants:
Inference Concerns

| | Burger King | | Domino's Pizza | |
	ln(franchise units)	ln(franchise units)	ln(franchise units)	ln(franchise units)
Termination restriction	−0.06 (0.04)	−0.06 (0.04)	−0.40 (0.04)***	−0.40 (0.10)***
State effects	yes	yes	yes	yes
Year effects	yes	yes	yes	yes
State trends	no	no	no	no
Clustering	state	multi-way (state and year)	state	multi-way (state and year)

Notes: *** $p < 0.01$ (against two-sided test of zero effect); ** $p < 0.05$ (against two-sided test of zero effect); * $p < 0.10$ (against two-sided test of zero effect). Sample includes the 50 states for the period 1990–2000 ($n = 550$) for Burger King and 1989–2000 for Domino's Pizza ($n = 600$). Regressions control for constant state per capita income. Franchise data come from UFOC's (item 20) filed with Maryland State Attorney General's Office.

We see that while the more sophisticated approach changes little with respect to Burger King, the Domino's coefficient's standard error increases by more than a factor of two. Despite this, however, the effect is still statistically significant.

A potentially larger problem involves statistical issues having to do with the very basis of statistical inference. The standard method of inference relies on asymptotic assumptions that may not be justified in small samples. Of particular concern here is the fact that our estimate of interest comes from a single law change. A single law change may not be sufficient to satisfy the assumptions supporting a central limit theorem justification for inference using the standard approach. If that is the case, inference in the standard way will only be valid if normality applies. This is obviously a very restrictive assumption.

Recent work by Conley and Taber (2011) and Gelbach, Helland, and Klick (2011) investigates this problem of inference based on a small number of policy changes. The presentations and suggested approaches are quite similar. Both rely on the fact that, under the null hypothesis of a zero effect, the residuals for the non-treatment observations provide a statistically valid empirical distribution of the treatment

effect coefficient under the null hypothesis. This empirical distribution can then be used to judge statistical significance based on how extreme the estimated treatment effect is relative to the residuals of the control observations.

The SQ test of Gelbach, Helland, and Klick (2011), which they develop in the event study framework, simply compares the point estimate of the event effect to the distribution of residuals for non-event observations. In the current context, we compared the immediate effect of the Iowa law (i.e., the estimated law coefficient restricting attention to 1992 and before) to the residuals from the model for all non-Iowa observations. Using a one-tailed test, we found that the size of the Iowa law effect on Burger King was more negative than all but the most extremely negative 2% of non-Iowa residuals. If, instead, we use a two-tailed test which simply compares the magnitude of the effect to the magnitude of the residuals, it was less extreme than 12% of the residuals.

For Domino's, a similar application of the SQ test finds the effect of the Iowa law to be more extremely negative than all but 2% of the non-Iowa residuals, while a two-tailed test places the Iowa effect as being more extreme, in absolute value, than 3% of the non-Iowa residuals.

The Conley and Taber approach, developed in the differences-in-differences context, estimates the treatment effect and compares it to a placebo effect occurring at the same time in each of the non-treatment entities. Using this approach, Burger King's response to the Iowa law was more extremely negative than 87% of the observed placebo effects. In absolute value terms, it was more extreme than 81% of the placebo effects. For Domino's, the comparable levels are 99% and 97%. These results are shown in Table 10.

Table 10
Effect of 1992 Iowa Termination Restriction on Franchised Restaurants:
Non-Parametric Inference

	Burger King ln(franchise units)	Domino's Pizza ln(franchise units)
Immediate effect of restriction	−0.91	−0.49
State effects	yes	yes
Year effects	yes	yes
State trends	no	no
SQ test 1-sided	rejects at 2nd percentile	rejects at 2nd percentile
SQ test 2-sided	rejects at 12th percentile	rejects at 3rd percentile
C&T test 1-sided	rejects at 13th percentile	rejects at 1st percentile
C&T test 2-sided	rejects at 19th percentile	rejects at 3rd percentile

Notes: SQ test refers to Gelbach, Helland, and Klick (2010) and uses the residuals for all states except Iowa and the District of Columbia for the periods 1990–2000 for Burger King and 1989–2000 for Domino's Pizza to construct the empirical test distribution. C&T refers to Conley and Taber (2011) and uses the 1992 residual for each state except Iowa and the District of Columbia to construct the empirical test distribution.

While the non-parametric approaches to inference reduce confidence slightly in the statistical significance of the effect of the Iowa law on Burger King, they confirm the statistical significance of the effect on Domino's. On the whole, these results suggest that the findings are unlikely to be the result of random variation.

6 Conclusion

While the franchise form has been studied extensively, the literature exhibits many limitations. First, the empirical work in this area is almost exclusively cross-sectional. This leads to significant problems of causal inference. Second, economists have paid too little attention to the legal environment in which franchise contracts are interpreted and enforced. This article makes improvements in both of these areas, using panel data on the franchise activity of some large fast-food chains to examine the effect of laws limiting the use of termination clauses in franchise contracts.

We also note that these termination laws can vary in their effect quite a bit from state to state depending on the degree to which parties can contract around the law. We show that there are very different effects of a termination law in Iowa that limited contractual avoidance of the law and a similar law in Washington, D.C. that did not limit these opportunities. Failure to recognize these differences limits the accuracy and the usefulness of previous work in the area. Some termination laws, specifically those that limit contractual work arounds, significantly affect franchise activity, while those that do not limit avoidance have ambiguous effects.

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Jonathan Klick
Law School
University of Pennsylvania
3400 Chestnut Street
Philadelphia, PA 19104
U.S.A.
E-mail:
jklick@law.upenn.edu

Bruce H. Kobayashi
School of Law
George Mason University
3301 Fairfax Drive
Arlington, VA 22201
U.S.A.
E-mail:
bkobayas@gmu.edu

Larry E. Ribstein
College of Law
University of Illinois
504 E. Pennsylvania Avenue
Champaign, IL 61820
U.S.A.
E-mail:
ribstein@illinois.edu