The Impact of Labor Mobility Restrictions on Managerial Actions:

Evidence from the Mutual Fund Industry

by

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Abstract

We examine how labor mobility restrictions such as non-compete clauses in employment contracts affect the behavior of employees. Using the mutual fund industry as a testing laboratory, we show that fund managers respond to increased enforceability of non-compete clauses by increasing their contribution to their employer's profitability. They do so by improving their fund performance, while also increasing window dressing to attract new customers in order to increase fee revenue. Furthermore, the resulting change in incentives disciplines managers' risk taking, as shown by noticeable reductions in their portfolio risk, portfolio deviations from their peers, and engagement in fund tournaments.

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In the last few years, there has been an intense debate in the U.S. surrounding labor mobility restrictions and their impact on economic activity (e.g., White House 2016 and U.S. Department of the Treasury 2016). A supporting argument is that by preventing employees from transferring intellectual property and skills acquired on-the-job to rival firms, such restrictions protect trade secrets and thus encourage innovation and investment in employee training. A counter argument, however, is that labor mobility restrictions limit the labor market pool from which companies can hire, which can result in suboptimal matching of talent with available jobs, prevent employees from founding new companies, and stifle innovation by reducing diffusion of knowledge and ideas among companies, all of which can potentially hinder economic growth.

Firms typically restrict labor mobility through non-compete clauses (NCCs) in employment contracts. Such clauses are heavily used in knowledge intensive industries (e.g., Starr, Prescott, and Bishara 2017) and for highly skilled and highly paid employees (e.g., Bishara, Martin, and Thomas 2015). They prohibit a separating employee from competing with her former employer, either by working for a competing firm or by establishing one on her own during a limited period of time and in a certain geographical area. For example, Bishara, Martin, and Thomas (2015) document that 80% of CEOs are bound by NCCs, often with a broad geographic scope, that generally last from one to two years. NCCs reduce the employee's opportunities in the external labor market and limit her bargaining power for more favorable future employment terms within the firm, both affecting the employee's future career.

While the literature is advancing in its understanding of the impact that NCCs have on economic growth, innovation, and investments at the regional and firm level, the analysis has typically abstracted away from the economic agents whose actions are directly targeted by these

labor restrictions.[1] The objective of our study is to fill this gap. In particular, we study how NCCs affect the behavior of labor force participants and their output.

Theory suggests that employees respond to implicit incentives, such as career concerns, in addition to explicit incentives resulting from the compensation contract. For example, Fama (1980) argues that labor market forces can solve agency problems and efficiently discipline managers to a higher effort level, even in the absence of explicit incentive contracts, while Holmstrom (1982, 1999) incorporates and enlarges upon this intuition in a formalized setting. Along these lines, building on the framework of Holmstrom (1999), Andersson (2002) shows that managers increase their effort when career concerns are present (relative to when they are absent) even if their compensation contract provides them with effort-based explicit incentives.

How NCCs affect the career concerns and behavior of employees is not obvious ex-ante. Since NCCs are typically enforced not only when an employee leaves the company voluntarily but also when that employee is fired, there are two opposing effects.[2] On the one hand, there is a disciplining effect: NCCs impose costs on the employees when they are fired because they have to stay unemployed for a certain period of time. This incentivizes them to increase their effort and consequently their output to avoid termination. On the other hand, NCCs reduce the outside options of employees in the external labor market, which makes it hard for them to exploit external promotion opportunities. This reduces their incentives to signal their quality to the external labor market and consequently makes them reduce their effort. Ultimately, it is an open empirical question which effect dominates.

[1] See, e.g., Bishara and Thomas (2015), Bishara and Starr (2016), and Prescott, Bishara, and Starr (2016) for recent reviews of the literature that looks at NCCs at the state and firm level.

[2] During our sample period, NCCs were enforceable in the U.S. even when an employee was fired in all but the following six states (see, e.g. Garmaise 2011): Arkansas, District of Columbia, Georgia, Kentucky, Maryland, and New Mexico. Thus, for all the states treated in our analysis, NCCs were enforceable when an employee was fired.

We use the mutual fund industry as a testing laboratory to examine the effect that NCCs have on the behavior of mutual fund managers and the resulting output they deliver. The mutual fund industry represents an ideal setting for our investigation for several reasons. First, since this industry is knowledge intensive and fund managers fit the income and industry profile of employees that are typically subject to such restrictions, we expect NCCs to be widely spread among mutual fund managers. This is indeed supported by empirical evidence presented later in the paper showing that changes in enforceability of NCCs affect labor mobility among mutual fund managers as expected. Second, for mutual fund managers, data availability allows us to directly observe their actions, i.e., their trades, as well as their production output, i.e., the performance of the funds for which they have responsibility. Third, given the relatively small number of players involved in the management of a mutual fund, we can more easily attribute production output to the actions of a mutual fund manager. The same cannot be said for corporations, where the output usually is the result of a complex network of interactions between a large set of production factors and economic agents. Finally, knowing not only mutual fund performance, i.e. the output fund managers deliver, but also the fund trades allows us to analyze the different ways in which fund managers respond to changes in NCC enforceability.

Our identification strategy for measuring the impact of NCCs exploits well-documented exogenous shocks to the enforceability of NCCs that happened in three states: Texas, Florida, and Louisiana. These three cases were first identified and employed in Garmaise (2011) and are commonly used in the NCC literature.[3] This setting helps us handle endogeneity concerns because these changes were introduced by state governments or Supreme Court rulings and were thus unlikely to be caused by fund or manager characteristics.

[3] Recent papers include Ali, Li, and Zhang (2018), Chen, Zhang, and Zhou (2018), He (2018)

Our first set of results unambiguously shows that increased enforceability of NCCs leads to better fund performance. This result holds regardless of whether we use different ways of measuring fund performance and employ control variables or not. The result is also economically significant, with increased NCC enforceability giving rise to performance improvement of affected mutual funds of more than 84 basis points per year, depending on the model we use. This result remains robust in a battery of robustness tests. Thus, the effort-increasing effect of higher costs when being laid off seems to be more important for fund managers than the effort-reducing effect arising from more-limited outside options.

After having established our main result, we dig deeper by taking into account that the relative importance of the two effects (higher costs associated with termination vs. more limited outside options) might differ between fund managers. For example, a fund manager who considers herself to be of low skill is likely to be more concerned about termination risk than about limited outside options relative to a fund manager who considers herself to be skilled. Therefore, we hypothesize that an increase in NCC enforceability increases the fund performance of the former manager more. Our empirical tests support this hypothesis. Furthermore, we hypothesize that concerns related to limited outside options are less important for fund managers employed by fund families with a larger internal labor market. The rationale is that families with a larger internal labor market offer fund managers more opportunities for promotion within the family, which should make the effort-reducing effect of limited outside options weaker. Thus, we would expect an increase in NCC enforceability to improve the performance of funds more if they belong to families with large internal labor markets. Again, our results support the hypothesis.

Finally, we focus on other actions fund managers might take in response to increased NCC enforceability. First, we look at actions of fund managers intended to make themselves useful to

the organization in a way that is unrelated to fund performance. In particular, we test whether fund managers do more window dressing—which helps attract new customers and thus generates additional fee income for the fund family—when NCCs become stricter. The rationale is that the additional fee income resulting from window dressing can mitigate the concerns that arise from stricter NCCs. On the one hand, it reduces the fund managers' risk of being fired, which is associated with higher costs in case of stricter NCCs. On the other hand, it increases the chances of managers being promoted in the internal labor market, which is more important in the case of limited outside options due to stricter NCCs. This is indeed what we find: Fund managers increase the amount of portfolio window dressing after an increase in NCC enforceability.

Furthermore, we hypothesize that stricter NCC enforceability causes fund managers to play it safe because they benefit less from taking risk when NCCs are stricter. If the risk taken leads to poor performance (in absolute terms or relative to their peers), fund managers might be fired and the costs associated with termination are higher in case of stricter NCC enforceability. At the same time, fund managers benefit less from risk taking even if it leads to great performance because they have limited outside options. Therefore, we hypothesize that in the face of increased NCC enforceability fund managers (i) reduce portfolio risk, (ii) engage less in tournament behavior, and (iii) herd more. We find evidence supporting all three hypotheses.

Our paper is related to a growing literature that studies the impact of NCCs on economic activity at the state and firm level. This literature looks at the effect of NCCs on the innovation process (e.g. Gilson 1999, Fallick, Fleischman, and Rebitzer 2006, Marx, Strumsky, and Fleming 2009, Samila and Sorenson 2011, Marx, Singh, and Fleming 2015, and Barnett and Sichelman 2016), entrepreneurship (e.g., Stuart and Sorenson 2003a, Stuart and Sorenson 2003b, Samila and Sorenson 2011, and Starr, Balasubramanian, and Sakakibara 2017), employee mobility (e.g.,

Fallick, Fleischman, and Rebitzer 2006, Marx, Strumsky, and Fleming 2009, and Jeffers 2018), firm-sponsored versus employee-paid training (e.g., Garmaise 2011, Starr, Prescott, and Bishara 2016, Starr 2018, and Starr, Ganco, and Campbell 2018), wages (e.g., Mukherjee and Vasconcelos 2011 and Balasubramanian, Chang, Sakakibara, Sivadasa, and Starr 2018), firms' output (e.g., Bishara 2011, Bishara and Orozco 2012, Lobel and Amir 2014, and Anand, Hasan, Sharma, and Wang 2018), as well as on the firms' financial reporting choices (e.g., Chen, Zhang, and Zhou 2018). Our paper contributes to this literature by furthering our understanding of how participants of the labor force respond to NCCs. Notably, our novel finding that NCCs have a disciplining impact on managers, inducing them to deliver better performance and temper their risk-taking behavior, contributes a new insight to the ongoing debate regarding the effect of NCCs on the economy.

Beyond the NCC literature, our paper also contributes to the literature that studies the career concerns of fund managers. Chevalier and Ellison (1999) document that managers with stronger termination sensitivity to performance play it safe by reducing portfolio risk and herding more. Brown, Harlow, and Starks (1996), followed by a large and growing number of studies, document that career concerns make mutual fund managers engage in tournaments, i.e. managers that performed poorly in the first part of the year increase their portfolio risk in the later part of the year to catch up with better performing competitors. Extending the Brown, Harlow, and Starks (1996) framework, Kempf, Ruenzi, and Thiele (2009) document that the relative importance of compensation and employment incentives determines to what extent fund managers will pursue tournament-like behavior, i.e., how they respond to mid-year performance by altering portfolio risk. In the context of this literature on the career concerns of fund managers, our paper is the first study to show how career concerns due to NCCs affect the output fund managers deliver to the

fund company. In particular, we document that stricter NCCs lead to better performance and more window dressing of fund managers, both increasing the fees earned by the fund company. Moreover, we show that increased career concerns cause managers to reduce their portfolio risk and tournament behavior and to increase their herding behavior, in effect confirming similar findings from the previous mutual fund literature. However, our distinct contribution is that we use an "exogenous shock" approach that allows us to draw causal inferences rather than inferences based on association.

The paper is organized as follows. In Section 1, we describe our data and methodology. In Section 2 we document that NCCs matter in the fund industry by showing that fund manager departure rates go down significantly when NCC enforceability become stricter. Section 3 presents the main result of our paper that an increase in NCC enforceability increases the performance fund managers deliver and shows for which fund managers this effect is particularly strong. Section 4 documents that fund managers also respond to increased NCC enforceability by doing more window dressing and taking less risk. Section 5 concludes.

1. Identification, Data, and Empirical Specification

1.1 Identification strategy

Our identification strategy exploits well-documented shocks to examine the causal effect of changes on NCC enforceability on our variables of interest. These changes took place in Texas, Florida, and Louisiana and were introduced by state governments or Supreme Court rulings and were thus unlikely to be caused by fund or manager characteristics. In all these states, NCCs apply to both cases, when an employee leaves the company voluntarily and also when the employee is fired.

In June 1994, Texas Supreme Courts redefined the legal standards for NCCs, making it more difficult to enforce NCCs.[4] For an NCC to be valid, the employment contract needed to explicitly mention the compensation the employee gets for signing the NCC. In late May 1996, Florida state legislature introduced a new law strengthening the employer's position enforcing NCCs. There were three major changes. First, there is a reversal of the burden of proof: the employee now has to prove that the NCC is not violated whereas before 1996 the employer had to prove the violation of the NCC. Second, courts must no longer consider "any individualized economic or other hardship that might be caused to the person against whom enforcement is sought".[5] Finally, even if the NCC states an overbroad time period or geographic range, the contract is no longer considered illegal but is applied in a modified version deemed as reasonable. Louisiana experienced two opposing changes. In June 2001, Louisiana Supreme Court effectively banned NCCs largely by voiding all agreements not pertaining to the case where the former employee seeks to establish a new business by herself.[6] However, in 2003 the former status quo was reestablished.[7]

A useful feature is that these changes have opposite effects on the enforceability of NCCs, i.e., increased enforceability for Florida and decreased enforceability for Texas. This way we can test the effect of an increase and a decrease of NCC enforceability separately, in effect using these opposite effects to check the construct validity of our main variable.

[4] *Light v. Centel Cellular Co. of Tex.*, 883 S.W.2d, 664-45 (Tex. 1994). https://www.courtlistener.com/opinion/201525150/light-v-centel-cellular-co-of-texas/

[5] Florida Sate Law §542.335(g)(1), http://www.leg.state.fl.us/statutes/index.cfm?App_mode=Display_Statute&URL=0500-0599/0542/Sections/0542.335.html.

[6] *SWAT 24 Shreveport Bossier, Inc. v. Bond*, 808 So. 2d 294 (La. 2001), http://caselaw.findlaw.com/la-supreme-court/1085030.html.

[7] See, e.g., Terrel (2004) and Ecker (2015).

1.2 Sample construction and data sources

Our sample period starts in 1992 and ends in 2004. The reason for this choice is that, as described above, during this period three states faced substantial amendments to their legal standards related to enforcement of NCCs, while NCC enforceability stayed constant in all the other states.[8]

Our sample incorporates several data sets. From the Center for Research on Security Prices (CRSP) Survivorship Bias Free Mutual Fund Database we get fund names, family names, monthly net returns, total nets assets under management, investment objectives, and further fund specific information such as expense and turnover ratios. For mutual funds with different share classes, we aggregate all observations at the fund-level based on the asset value of the share classes. We limit the universe to include only diversified, domestic U.S. equity funds, thereby excluding index, balanced, bond, money market, and sector funds. The portfolio holdings data come from the Thomson Financial Mutual Fund Holdings database, which we merge with the CRSP mutual fund data using the MFLINKS database and with the CRSP stock data using stock CUSIPS. Portfolio holdings for each fund are either of quarterly or semi-annual frequency.

NCC enforceability is governed by state law and changes in NCC enforceability take place at the state level. To determine the relevant state, we rely on N-SAR filings by mutual funds, which we retrieve through the Electronic Data Gathering, Analysis, and Retrieval (EDGAR) system maintained by the SEC. We download all N-SAR A and B filings and match them to our CRSP sample funds by name. The fund managers conducting the actual asset management are employees of the fund's "advisor" (item #8) and the advisor's "state of headquarter" (item #8.D), as opposed to the state of incorporation, is the relevant state, the laws of which govern the pertinent NCC law

[8] Our choice is consistent, among others, with Garmaise (2011) and Chen, Zhang, and Zhou (2018), who use the same sample period. Moreover, like us, these studies use the amendments to NCC enforceability outlined in Section 1.1.

applicable to the fund managers.[9] Each fund with one unique advisor state is assigned one distinct NCC jurisdiction; we exclude all other funds without a unique advisor state from the sample.

1.3 Descriptive Statistics

Our sample from 1992 to 2004 includes 2,063 funds managed by 3,396 distinct managers. Out of the sample funds, 110 (5.3%) are from one of the treated states (Texas = 73 funds, Florida = 34 funds, Louisiana = 3 funds). Similarly, out of the 3,396 sample managers, 198 (5.9%) come from a state experiencing a change in NCC enforceability (Texas = 146 managers, Florida = 48 managers, Louisiana = 4 managers).

[please insert Table I here]

Table I provides descriptive statistics for the total sample as well as the treated and untreated subsamples separately. The average fund has almost $1 billion in assets, has an annual expense ratio of 1.4%, and turns over its portfolio approximately once per year (mean turnover ratio of 96%). On average, sample funds are 12 years old. The mean annual flow, defined as the percentage growth in assets under management not attributable to fund performance, reflects the large overall growth of the active mutual fund industry during this period. The average sample family has about 24 billion in assets, manages 14 funds, and employs roughly 16 managers. In terms of assets as well as flows, funds from the control and treated group are largely comparable. Treated funds exhibit slightly lower turnover than the control group (89% versus 96%). They are also two years older and charge 16 basis points higher in fees. Consistent with states in the control

[9] This was most recently confirmed in *Ascension Insurance Holdings, LLC v. Underwood et al.* (January 28, 2015), whereby the Delaware Court of Chancery concluded that California law must be applied with respect to a non-compete agreement signed by a California-based employee despite a Delaware choice-of-law provision contained in the non-compete agreement.

group housing large financial centers (e.g., New York, California, and Pennsylvania), which host disproportionately more large families, families from treated states are significantly smaller both in terms of totals assets and number of funds managed but not in terms of number of managers employed. Besides these differences, there are no other discernible differences between the subsamples.

1.4 Methodology

To test the hypotheses developed in the introduction, we estimate a generalized difference-in-differences regression model that resembles the one employed by Bertrand and Mullainathan (2003) to examine the effects of anti-takeover law changes:

$$y_{i,s,t} = \beta_0 + \beta_1 \cdot Treated_{i,s,t} \cdot Post_t + Controls + FE + \varepsilon_{i,t}, \quad (1)$$

where $y_{i,s,t}$ is the variable of interest for fund *i* from state *s* in period *t*. Following Garmaise (2011), we use the changes in the legal environment detailed above to generate our main independent variable, $Treated_{i,s,t} \cdot Post_t$, and assume that the legal changes affect managerial behavior starting in the year following their occurrence. Accordingly, this variable is set to -1 for funds in Texas from 1995 to 2004, +1 for funds in Florida from 1997 to 2004, and -1 for funds in Louisiana in 2002 and 2003 and is set to 0 otherwise. *Controls* denotes fund-level control variables, which are described in Table I.[10] In particular, we include the fund's expense ratio (*Expense Ratio*), turnover ratio (*Turnover Ratio*), flows (*Flow*), age (*Log[Age]*), and total net assets (*Log[TNA]*). *FE* denotes various fixed effects. We use fund fixed effects to control for time-invariant differences

[10] We also calculate results without control variables to address the potential concern that the change in NCC enforceability might have an impact on the time-varying controls and lead to inconsistent estimates of the treatment effect. However, the results clearly show that this is not the case. The treatment effect is essentially the same with and without control variables.

between treated and non-treated funds, time fixed effects to account for common time variant factors, and style fixed effects to control for commonalities within investment styles. We cluster standard errors at the state level in all specifications.

2. Do NCCs Matter in the Fund Industry?

NCCs are very common in knowledge-intensive industries (e.g., Starr, Prescott, and Bishara 2017).[11] Since mutual fund families almost exclusively consist of human capital (e.g. Berk, van Binsbergen, and Liu 2017), there is a strong rationale for the use of NCCs in the fund industry. They are intended to help with talent retention and keep fund managers from disseminating any trade secrets related to investment processes, investment strategies, and trading algorithms to competitors. In addition, another rationale for investment firms to use NCCs is to keep their portfolio managers from taking the firms' clients with them when they join a competitor or start their own firm.

There are no requirements for investment firms such as mutual fund families and affiliated entities to report information on the use and details of NCCs for their fund managers, thus detailed data on their use is unavailable. However, we can provide evidence on the use of NCCs in the mutual fund industry by documenting that labor mobility in that industry declines when NCC enforceability increases. To do so, we calculate the departure rate of a fund company's fund managers in year *t* as the number of fund manager departures in that year scaled by the number of fund managers in that company. We find that the average annual departure rate in the treated states is 7% in our sample period. Looking separately at the years before and after changes in NCC

[11] The reach of NCCs has moved beyond high skill, high paying occupations, however, in recent years. Dougherty (2017) reports that in the last few years there has been a significant increase both in the use of NCCs by companies (to cover even non-technical workers such as sandwich makers and hairstylists) and the number of NCC lawsuits brought by companies.

enforceability, we find that the annual departure rate went down from 22% to 3% in Florida where NCC enforceability increased and went up from 3% to 14% in Texas where NCC enforceability decreased. In a more formal test, we apply the generalized diff-in-diff approach (1) and use the departure rate as the dependent variable.

[please insert Table II here]

Table II clearly shows that the departure rate drops when NCCs enforceability becomes stricter. This holds, independent of whether we estimate the model with or without control variables. In both cases, the drop in departure rates due to increased NCC enforceability is about 6 percentage points, which is highly significant in statistical and economic terms. This suggests that NCCs are used considerably by mutual fund families and that NCCs indeed affect the mobility of mutual fund managers. Since we are unable to observe the employment contracts of the fund managers, we cannot rule out that some fund managers in the treated states have no NCCs in their contract and therefore are unaffected by changes in the NCC enforceability. However, this would create an attenuation bias in our sample, which would make it more difficult to find significant results throughout the paper.

3. The Impact of NCCs on Fund Managers' Performance

In this section, we analyze how changes in NCCs affect the performance that fund managers deliver. As outlined in the introduction, there are two opposing effects. On the one hand, NCCs incur costs to employees when they are fired, thus incentivizing them to work harder and deliver better performance in order to avoid termination. On the other hand, NCCs limit the outside options of fund managers in the external labor market and thus reduce their incentives to work hard in order to achieve very good performance and make themselves marketable in the external

job market. In Section 3.1 we show that the disciplining effect dominates and that an increase in NCC enforceability makes fund managers deliver better performance. In Section 3.2, we run various additional tests to provide further support for this finding and rule out alternative explanations. Finally, in Section 3.3 we analyze whether the relative importance of the two effects described above depends on characteristics of the fund manager and the fund family.

3.1 Main Result

To test how changes in NCC enforceability affect the performance of fund managers, we use model (1), but now with monthly fund performance as the dependent variable. We employ four measures of fund performance: raw return (*Return*); style-adjusted return (*Style-adj. Return*); characteristic-adjusted return measure of Daniel, Grinblatt, Titman, and Wermers (1997) and Wermers (2004) (*DGTW*); and Carhart (1997) 4-factor alpha (*Carhart*). To measure style-adjusted returns, we subtract from the return of a given fund the mean return of all funds belonging to the same investment category. We calculate characteristic-adjusted returns as follows. First, we determine a portfolio stock's characteristic adjusted return in a given month by subtracting from its return the return of the benchmark portfolio, which that particular stock belongs to.[12] Each stock's benchmark portfolio is a value-weighted portfolio that includes all stocks that are part of the same size, book-to-market, and one-year past return quintile. Then, we calculate a fund-level *DGTW* measure as the value weighted sum of stock-level characteristic-adjusted returns. As our last measure of fund performance, we calculate a fund's 4-factor alpha as the difference between the actual return and the expected return, estimated using factor loadings computed from a

[12] The DGTW benchmarks are available via http://www.smith.umd.edu/faculty/rwermers/ftpsite/Dgtw/coverpage.html.

regression of the preceding 36 monthly excess returns on the respective factor-mimicking portfolios.[13]

[please insert Table III here]

Table III reports regression results with our four performance measures as dependent variables, both with and without control variables included. The results provide strong evidence that increased enforceability leads to improved performance. For each performance measure, the coefficient of $Treated \cdot Post$ is statistically significant at the 1% level and its magnitude implies a large economic impact. For example, results based on DGTW without controls indicate that a change toward stricter enforcement of NCCs leads to an alpha increase of 13 basis points per month, which corresponds to a 156 basis points improvement on an annual basis. Looking at all the performance measures suggests that the shift towards stricter NCC enforceability leads to a performance improvement that ranges from 156 to 260 basis points per year. When we include control variables, the coefficients decline by up to a third of the values obtained without controls, although they remain statistically significant at the 1%-level. In sum, the results from Table III suggest that an increase in NCC enforceability makes fund managers deliver a better performance. This is consistent with the view that the effort-increasing effect of higher costs associated with being fired is more important for fund managers than the effort-reducing effect of limited outside options.

The reliability of causal inferences obtained from our difference-in-differences estimation hinges on the assumption that, in the absence of changes in NCC enforceability, changes in our variables of interest are the same for the treated and control group, i.e., these variables exhibit

[13] Returns for the factor mimicking portfolios and the proxy for the risk-free rate are available via http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.

trends that are parallel between the treated and control groups before the treatment. To check the validity of this assumption, we augment model (1) with a term that interacts an indicator variable for the year prior to the change in NCC enforceability (*Pre*) with the treatment dummy. Given that the treatment for Texas funds starts in 1995 and our sample starts in 1992, we chose to look at only one year prior to the treatment so that our benchmark period covers at least two years.

[please insert Table IV here]

Results for regressions with and without control variables are presented in Table IV. They show that for none of the performance measures the interaction term, $Treated \cdot Pre$, is economically or statistically significant while it continues to be significant for $Treated \cdot Post$. This evidence supports the assumption that treated and the control group exhibited parallel trends prior to changes in the NCC enforceability and that these changes served as an exogenous shock on the effort of fund managers.

3.2 Robustness Tests

To provide further support for our main finding in Table III and to rule out alternative explanations, we run various additional tests.

3.2.1 Single-State Analysis

Model (1) staggers changes in NCC enforceability in both directions, that is, it includes both increases and decreases in NCC enforceability. However, our data allows us to discern how fund managers react to opposite changes in NCC enforceability by looking separately at the effects of increased enforceability and decreased enforceability. Therefore, in our first robustness test, we

re-estimate our main result separately for Florida (where NCC enforceability increased) and Texas (where NCC enforceability decreased).[14]

To run this test, we replace the variable *Treated · Post* in model (1) with *Increased · Post,* which equals +1 for funds from Florida during 1997-2004 and 0 otherwise. When focusing on Texas, we replace the variable *Treated · Post* with *Decreased · Post*, which equals +1 for funds with advisors headquartered in Texas from 1995-2004 and 0 otherwise. We hypothesize that increased or decreased enforceability of NCCs leads to effects on the left-hand side variable that have opposite signs. Table V presents results. We report results only for the model with control variables but we get very similar results when we estimate the model without controls. In the interest of brevity, we report only the main coefficients of interest.[15]

[please insert Table V here]

Table V clearly documents the robustness of our main result. All relevant coefficients have the hypothesized signs. For Florida, where NCC enforceability increased, we find that fund managers deliver better performance. In contrast, for Texas, where NCC enforceability decreased, we find the opposite effect. Regarding statistical and economic significance, the results are on par with the results of the aggregated analysis. This is sensible since the magnitude of the change in enforceability, as shown by Garmaise (2011), is the same in absolute terms for both states. Thus, our main finding does not only hold when we jointly look at all changes in NCC enforceability but also for each change in NCC enforceability separately. This increases the confidence that the effect we are documenting indeed results from changes in NCC enforceability.

[14] We are unable to conduct a similar analysis for Louisiana given that there are only three treated funds in Louisiana.
[15] Tables including the control variables as well as tables for the model without control variables are available from the authors upon request.

3.2.2 Investments in the Home State

As documented by Coval and Moskowitz (1999, 2001), fund managers tend to overweight local stocks. Since changes in NCC enforceability affect not only fund managers but also CEOs and other managers of companies operating in the same state, one might argue that our result is driven by changes in the behavior of managers of local companies held in the fund portfolios rather than by changes in the behavior of fund managers. Local company CEOs might work harder due to increased NCC enforceability, which might drive the performance results documented above.[16]

To address this concern, we split the portfolio of a fund manager into two sub-portfolios. The local sub-portfolio consists of all holdings of companies that are located in the same state as the fund manager, the non-local sub-portfolio consists of all other holdings. For these sub-portfolios we determine quarterly portfolio performance (based on returns, DGTW returns, and 4-factor alphas).[17] We then test whether impact of $Treated \cdot Post$ on performance is stronger for local holdings than for non-local holding. The results are presented in Table VI.

[please insert Table VI here]

Table VI documents that the effect of $Treated \cdot Post$ on performance is not stronger for local holdings than for non-local holdings. This rules out that our main result reflects behavioral changes of managers of local companies, providing further confidence in the inferences that we draw from our main result.

[16] For example, Berger, Ofek, and Yermack (1997), Betrand and Schoar (2003), Adams, Almeida, and Ferreira (2005), and Bennedsen, Pérez-González, and Wolfenzon (2018) show the influence of CEOs on stock performance.
[17] We cannot calculate style-adjusted returns because the analysis is now at the stock-level whereas style is a characteristic of the fund.

3.2.3 Switching Fund Managers

Manager turnover is another alternative explanation for our main result. Some managers might self-select to join or leave fund families that are affected by changes in NCC enforceability. To rule out this alternative explanation, we re-run our main test using only the subsample where the same fund manager was responsible for the fund before and after the change in NCC enforceability.

[please insert Table VII here]

The results presented in Panel A of Table VII rule out the alternative explanation that fund manager changes caused by self-selection drive our results. Using only constant manager-fund combinations, our result remains qualitatively unchanged.

However, even for managers remaining with the same fund after the change in NCC enforceability, one might argue that their employment contracts are adjusted by the fund family after the change in NCC enforceability. This raises the possibility that the treatment effect we document does not fully come from changes in the implicit incentives of the fund managers but also from changes in their explicit incentives.[18] The unobservability of employment contracts makes it impossible for us to check whether and how the contracts actually change. Nonetheless, contract adjustments cannot be instantaneous given the discussions and negotiations expected to take place between the parties involved. For this reason, we expect contract changes to be less relevant when we look at shorter post-treatment periods, a consideration which leads us to use

[18] It is not clear ex-ante how exactly fund companies would change the compensation contracts. It is plausible, however, that the change will reflect the belief of the fund company as to whether the disciplining effect or the effort-reducing effect will dominate the behavior of a given manager after the change. If the fund company believes that the effort-reducing effect will dominate, it might want to make that manager's compensation more performance-sensitive. On the other hand, the fund company might make the opposite change to the compensation contract if it thinks that the disciplining effect will dominate.

post-treatment periods ranging from three to one years, respectively, in Panels B – D of Table VII.

Evidence from these panels shows that our main result holds even when we use shorter post-treatment periods. This suggests an immediate response of fund managers to changes in NCC enforceability, making it less likely that our results are driven by adjustments of the compensation contracts of fund managers.

3.2.4 Propensity Score Matching

So far, we consider all untreated funds as the control group and take differences between treated and untreated funds (as documented in Table I) into account by employing fund characteristics as control variables in our regressions. However, it could be that the results might reflect differences in fund characteristics that the linear model does not properly control for. To address this concern, we use a matching approach that selects the control group to consist of only non-treated funds that are matched to the treated funds. More specifically, we run a logistic regression that relates the probability of a fund being treated to fund characteristics. Using the propensity score from this regression, we determine the nearest untreated neighbor for each treated fund. We then re-run our main test using only the treated and the matched untreated funds.

[please insert Table VIII here]

The results of Table VIII rule out the possibility that the way we control for differences in fund characteristics is responsible for our main result. When we use a propensity score matching approach, our main result remains unchanged.

In summary, the findings of Section 3.2 provide further support for our main result and help rule out various alternative explanations for it.

3.3 Cross-Sectional Differences in the Behavior of Fund Managers

After having established our main result, we examine factors that we hypothesize to affect the relative strength of the two opposing effects, i.e., the effort-increasing effect caused by costs associated with termination vs. the effort-reducing effect due to limited outside options. To this end, we consider managerial skill in Section 3.3.1 and size of the internal labor market in a fund family in Section 3.3.2.

3.3.1 Low- versus High-Skilled Managers

We expect that the relative importance of the two effects associated with an increase in NCC enforceability depends on a fund manager's perception of her skill. A fund manager who considers herself to be of lower skill is likely to be more concerned about termination risk than about limited outside options relative to a fund manager who considers herself to be more skilled. Therefore, we hypothesize that the performance impact of increased NCC enforceability is stronger for less-skilled than for more-skilled managers.

We measure a manager's perception of her skill as the average SAT score of matriculates at the institution where the manager obtained her bachelor degree. We first collected information on which universities managers obtained their degree from using Morningstar Direct, Morningstar Principia, SEC filings, LinkedIn, and the websites of the fund companies. Then, from the College Scorecard provided by the U.S. Department of Education, we obtained the average SAT scores of the institutions from where managers graduated.

To test the prediction that an increase in NCC enforceability leads to greater performance increases for lower-skilled managers, we augment model (1) with two variables: SAT, the average SAT score of the school from which the manager graduated, and the interaction of

$Treated_{i,s,t} \cdot Post_t$ with SAT. Consistent with our discussion above, we expect the interaction term to be negative.

[please insert Table IX here]

The results provided in Table IX support our hypothesis. The coefficient on the interaction term is negative and statistically significant. This suggests that the performance improvement due to increased NCC enforceability intensifies for lower-SAT managers. This is consistent with the view that for managers with lower skill, the effort-increasing effect due to higher costs associated with termination dominates the effort-reducing effect due to limited outside options to a greater extent than for managers with higher skill.[19]

3.3.2 Managers in Families with Large versus Small Internal Labor Markets

We next test whether the reaction of fund managers to changes in NCC enforceability depends on how developed the internal labor markets are in which managers operate. Managers that work for larger fund families are expected to benefit from the presence of more developed internal markets, which allow them to replace restricted across-family mobility with within-family mobility (Papageorgiou 2014, 2018). In more developed internal markets, managers still face the risk of being fired, but at the same time, they have more opportunities to be internally promoted. Thus, the managers' concerns of a limited upside after increased NCC enforceability are mitigated in larger families. This weakens the relative importance of the effort-reducing effect in favor of

[19] We also examined whether increases in NCC enforceability have a different performance impact for younger vs. older managers. Chevalier and Ellison (1999) argue that the likelihood of being fired due to poor performance is higher for younger managers than for older managers. This suggests that an increase in the cost of being fired due to stricter NCC is more relevant for younger managers than for older managers. At the same time, an opposing effect is also likely: limited outside options due to stricter NCCs are likely to be more important for younger managers since they are just starting their career in the fund business. Unreported results suggest that manager age does not affect the performance impact of changes in NCC enforceability significantly, which is consistent with the two effects offsetting each other.

the disciplining effect. Therefore, we expect the positive performance effect of increased NCC enforceability to be greater in larger families than in smaller families.

We measure the size of the internal labor market in a fund family by the number of family funds. To quantify the effect of family size on the performance impact of changes in NCC enforceability, we augment equation (1) with two variables: FamilySize, the number of family funds, and the interaction of $Treated_{i,s,t} \cdot Post_t$ with FamilySize. Consistent with our discussion above, we expect the interaction term to be positive.

[please insert Table X here]

Results from this regression are reported in Table X. They show that, as expected, family size interacts positively with $Treated_{i,s,t} \cdot Post_t$. Except for the DGTW specification, the coefficient of the interaction term is statistically significant at the 1 percent level in all specifications. Overall, the evidence from Table X supports our hypothesis: Fund managers in larger families increase their performance more than fund managers in smaller families following increased enforceability of NCCs. This is consistent with the notion that the effort-reducing effect of limited outside options is less important in larger families where managers have more opportunities to be promoted internally.

4. Other Actions of Fund Managers in Response to NCCs

So far, we have shown that one action managers take in response to increased NCC enforceability is to increase their output, i.e., deliver better performance. This makes fund managers more valuable for their fund families and thus reduces their likelihood of being fired. In this section, we document other actions fund managers take that are not intended to deliver a better performance.

In Section 4.1, we document that managers also make themselves valuable to the fund family by window dressing their portfolios, which can help attract new money and consequently generate more income for the fund family. In Section 4.2, we show that, consistent with the finding of Chevallier and Ellison (1999) and Kempf, Ruenzi, and Thiele (2009) that fund managers respond to career concerns by taking less risk, fund managers reduce their risk taking in response to increased NCC enforceability.

4.1 The Impact of NCCs on Window Dressing

We hypothesize that fund managers increase window dressing to attract new customers and thus additional fee income for the fund family when NCCs become stricter. The rationale is that by attracting new customers and thus inflating assets under management, fund managers can directly improve the profitability of their fund family and consequently increase their standing and job security in the fund family (e.g., Agarwal, Gay, and Ling 2014).[20] At the same time, this higher contribution of fund managers to the profitability of the company increases their chances of being promoted in the internal labor market, which is more important when outside options due to stricter NCCs are limited.

To test this hypothesis, we estimate model (1) with the two measures of window dressing developed by Solomon, Soltes, and Sosyura (2014) and Agarwal, Gay, and Ling (2014) as dependent variables. The first one, *Rank Gap,* measures the gap between a fund's return rank and a rank based on its stock holdings. The latter is calculated as the average of a rank based on the proportion of winners (the higher the proportion of winners, the higher the rank) and losers (the lower the proportion of losers, the higher the rank). The intuition is that if a fund's return was low

[20] Seminal papers on window dressing by institutional investors are Lakonishok, Shleifer, Thaler, and Vishny (l991) and Sias and Starks (1997).

relative to other funds, despite its portfolio covering a relatively high amount of winners and low amount of losers, this is interpreted as evidence of window dressing. The second measure of window dressing is the *Backwards Holding Return Gap (BHRG)*.[21] It is measured as the difference between the quarterly return, net of expenses and trading costs, of a hypothetical portfolio consisting of a fund's end-of-quarter holdings assumed to have been held through the whole quarter up until the next report date and the fund's actual quarterly return. As with *Rank Gap*, high values of *BHRG* indicate that reported holdings suggest higher returns than actually realized, consistent with window dressing. Results from these regressions are presented in Table XI.

[please insert Table XI here]

In Table XI, the positive coefficients of $Treated \cdot Post$, statistically significant at the 1%-level, are consistent with managers increasing their window dressing behavior after an increase in NCC enforceability. These results are also economically significant. For example, a coefficient of 0.0035 in the regression using *Backward Holding Return Gap* as the dependent variable corresponds to an increase in window dressing behavior that amounts to 61% of the sample mean for *Backward Holding Return Gap*. High economic significance for this effect is also observed in the regression that uses *Rank Gap* as the dependent variable. This finding suggests that fund managers respond to increased NCC enforceability by making themselves more useful to the fund family in ways that go beyond changes in fund performance. In particular, they do so by increasing window-dressing, most likely done to attract more investor flows and generate higher fee income for the fund family.

[21] Other studies that use *BHRG* include Brown, Sotes-Paladino, and Jiaguo (2017), and Chuprinin and Sosyura (2018). Bai, Ma, Mullally, and Solomon (2018) additionally employ *Rank Gap*.

4.2 The Impact of NCCs on Risk Taking

We now move to our final hypothesis: stricter NCC enforceability causes fund managers to play it safe by (i) reducing portfolio risk, (ii) deviating less from their peers, and (iii) engaging less in tournament behavior. The rationale underlying this hypothesis is that fund managers benefit less from taking risk (in absolute terms or relative to their peers) when NCCs are stricter. If the risky actions that fund managers take lead to poor performance, fund managers run the risk of being fired and the costs associated with termination are higher in case of stricter NCC enforceability. At the same time, when NCC enforceability is stricter, fund managers benefit less from risky actions even if they lead to great performance simply because their outside options are more limited.

To test the predictions (i) and (ii), we run model (1) with risk and herding measures as dependent variables. These are volatility (*Volatility),* return semi-deviation (*Semi-Deviation*), downside beta (*Downside-β*), and portfolio herding (*Herding*). We compute return volatility as the standard deviation of a fund's past twelve months' net returns. The next two variables measure downside risk. *Semi-Deviation,* which also uses a fund's past twelve months' returns, reflects deviations from the mean for returns that were below the mean. We follow Whaley (2002) and compute downside beta based on the covariance with the market only when the excess fund and market returns are both below the zero threshold. To compute *Herding*, we first calculate a stock-level based herding measure following Lakonishok, Shleifer, and Vishny (1992), which we then aggregate at the fund level by value-weighting it over all stocks in a fund's portfolio. The results of our regressions are presented in Table XII

[please insert Table XII here]

Panel A of Table XII shows that, as expected, increased enforceability of NCCs leads to a decrease in portfolio risk taking and an increase in herding. Specifically, an increase in NCC enforceability leads to a decrease in *Volatility* of 15 basis points, in *Semi-Deviation* of 36 basis points, and a decrease in *Downside-β* of more than 0.09, which are all sizable relative to the sample means of these variables. For example, the decrease of 0.09 in *Downside-β* corresponds to a reduction of about 9% of the sample mean. The positive and significant coefficient of $Treated \cdot Post$ in regressions with *Herding* as dependent variable suggests that an increase in NCC enforceability leads to more herding by the affected fund managers. This increase in herding appears to be of striking economic magnitude, in that the coefficient of $Treated \cdot Post$ amounts to 172% of the sample mean for the *Herding* measure. Thus, evidence from Panel A clearly supports hypotheses (i) and (ii).

Managers investing much more like their peer group appears to be at odds with the fact that a fund manager needs to deviate from her peers to outperform them. In unreported tests, we find that fund managers respond to increased enforceability by taking fewer risky bets relative to peers, which is consistent with increased herding, while at the same time the risky bets they take deliver a better performance, consistent with the view that they exert more effort in identifying promising investment ideas behind these risky bets.[22]

We now move on to our tournament hypothesis (iii). Brown, Harlow, and Starks (1996) are the first to examine the risk taking incentives of fund managers in a tournament setting. They show that fund managers with poor interim performance increase their risk taking in the second half of the year to catch up with the interim winners. However, since an increase in NCC enforceability increases the cost of being fired, we expect fund managers to cut back on their tournament-driven actions in the

[22] The corresponding table is available from the authors upon request.

face of increased NCC. To test this hypothesis, we use the risk adjustment ratio of Kempf, Ruenzi, and Thiele (2009) as the dependent variable in our regression:

$$RAR_{i,s,t} = \frac{\sigma^{(2),\text{int}}_{i,s,t}}{\sigma^{(1)}_{i,s,t}} \tag{2}$$

RAR captures how much fund managers change their risk in the second half of the year relative to the first half. $\sigma^{(1)}_{i,s,t}$ denotes the realized portfolio risk of fund *i* in state *s* in the first half of year *t*. It is calculated using the actual portfolio holdings and the actual volatility of the corresponding portfolio returns in the first half of the year. The intended portfolio risk, $\sigma^{(2),\text{int}}_{i,s,t}$, in the second half of year *t* is calculated using the actual portfolio holdings in the second half and the forecast of the volatility of the corresponding portfolio returns in the second half of the year (which is proxied by the realized stock volatility of that same portfolio in the first half of the year).[23] Our regression model to test for the impact of changes in NCC enforceability on tournament behavior reads as:

$$\begin{aligned} RAR_{i,s,t} = {} & \beta_0 + \beta_1 \cdot Perf^{First}_{i,s,t} + \beta_2 \cdot Treated_{i,s,t} \cdot Post_t + \beta_3 \cdot Perf^{First}_{i,s,t} \cdot Treated_{i,s,t} \cdot Post_t \\ & + Controls + FE + \varepsilon_{i,t}, \end{aligned} \tag{3}$$

$Perf^{First}_{i,s,t}$ denotes the performance of the fund *i* in state *s* during the first half of year *t*. We measure performance as the style-adjusted returns or as ranks based on raw returns. Ranks are calculated for each market segment and year separately. They are normalized so that they are equally distributed between zero and one, with the best fund manager in its respective segment getting assigned the rank of one. According to the traditional tournament literature, we expect a

[23] This approach for measuring intended risk is the same as the one used in Kempf, Ruenzi, and Thiele (2009) and Ma, Tang, and Gómez (2018). For more details, please see the appendix of Kempf, Ruenzi, and Thiele (2009).

negative coefficient on $Perf_{i,s,t}^{First}$ ($\beta_1 < 0$), i.e. the lower the performance in the first half of the year, the more fund managers increase risk in the second half of the year. Like in model (1), $Treated \cdot Post$ captures the change in the enforceability of NCCs, *Controls* denotes fund-level controls and *FE* the various fixed effects. The main variable of interest is the interaction term. The coefficient β_3 shows how a change in enforceability impacts tournament behavior. Since we expect that fund managers engage less in tournaments when enforceability of NCCs is increased, we expect β_3 to have to opposite sign of β_1, i.e., $\beta_3 > 0$.

Panel B of Table XII presents the results. It clearly shows that an increase in NCC enforceability mitigates the tournament behavior of fund managers. Whereas we observe a general tendency for tournament-like behavior ($\beta_1 < 0$), we see that this behavior changes when NCC enforceability is increased ($\beta_3 > 0$). This change is strong, both in statistical and economic terms. β_3 is statistically significant at the 1%-level in each model specification. Furthermore, the size of β_3 is larger than the size of β_1 in absolute terms, i.e. the change effect dominates the baseline tournament effect. This implies that an increase in NCC enforceability prevents fund managers from engaging in tournaments. They no longer increase the risk of their portfolio in response to poor interim performance but instead play it safe. This finding again highlights the importance of career concerns resulting from NCCs.

In summary, the results on Table XII clearly show that fund managers play it safe when NCC enforceability is increased. This is highly sensible since increased NCC enforceability leads to higher potential costs and lower potential benefits of risk taking.

4. Conclusion

In the last few years, non-compete clauses in employment contracts, intended to restrict labor mobility, have received growing attention from academics, regulators, politicians, companies, and the public at large. While the focus of this debate has been on how these restrictions affect overall state or firm economic activity, we know little about how the targeted members of the labor force respond to such restrictions. Our paper contributes to this ongoing debate through a unified examination of the effect that NCCs have on the behavior of managers by looking at a number of possible actions that managers can undertake.

Using the mutual fund industry as a testing laboratory, we show that managers respond to increased NCC by increasing their contribution to the profitability of their fund company. They do so by improving their performance, while at the same time they increase window dressing in order to attract new investor flows and consequently earn higher income for the fund company. In addition, we find that stricter NCCs discipline managers' risk taking, as shown by noticeable reductions in their portfolio risk, portfolio deviations from their peers, and engagement in fund tournaments. All these behavioral changes of fund managers are highly sensible given that stricter NCCs increase their costs associated with being fired and reduce their options in the external labor market.

Our findings also inform the regulatory debate regarding possible courses of action with respect to enforceability of NCCs by providing a micro-view on how employees adjust their behavior in response to changes in NCC enforceability. Both fund management companies and fund investors appear to benefit from how fund managers react to increased NCC enforceability. Fund management companies benefit from an alignment of incentives, whereby employees increase contribution to the firm's profitability. This alignment of incentives also proves beneficial

to fund investors who gain from the improved fund performance. At the same time, employees face greater incentives to reign in their risk-taking behavior, which can contribute to reduced systemic risk. Whether and to what extent similar effects extend to other important financial institutions such as banks, insurance companies, or hedge funds is an interesting venue for future research.

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Appendix: NCCs in the investment industry

Since human capital is one of the most important means of production for mutual fund families (e.g. Berk, van Binsbergen, and Liu 2017), the main rationale for utilizing NCCs by mutual fund management companies is to retain talent. In addition, by restricting employee mobility, fund families hinder dissemination of their organization knowledge to competitors and also keep their portfolio managers from taking the firms' clients with them when they join a competitor or start their own firm.

There are no requirements for investment firms such as mutual fund families and affiliated entities to report information related to their use of NCCs, thus detailed data on which of their employees are subject to NCCs and under what terms is unavailable. Nonetheless, given the human capital- and knowledge-intensive nature of the mutual fund industry, there are a number of indications that NCCs are commonly used in this industry. There is some indirect evidence that comes from the survey of Starr, Prescott, and Bishara (2017). Although the survey does not single out mutual fund managers, these individuals fit the income and industry profile of employees that the survey shows to be typically subjected to such restrictions. For example, Starr, Prescott, and Bishara (2017) document that employees in the highest income bracket ($150K+) have the highest incidence rate, as high as about 60%, of being subjected to NCCs. Moreover, the broader industry in which they work, i.e., financial services, is close to the top 20% of industries with the highest incidence rate of NCCs.

There is also some direct, albeit rather limited, evidence in the public domain, which is primarily available through business press coverage, on the terms and use of NCCs by investment firms. One high profile example is the case of Jack Bogle, former CEO of Vanguard Group and a highly influential figure in the mutual fund industry. He was subject to a NCC with the Wellington

Management Company after leaving in 1974 to found the Vanguard Group. The outstanding NCC restricted Bogle from entering the active fund management business, but it did not apply to passive management, which allowed Bogle to introduce the first index fund (Regan 2016).

Most of the related evidence on the use of NCCs discussed in the business press is from coverage of lawsuits filed by investment firms against their former fund managers for breach of their NCCs. In this context, a notable case was the lawsuit by Wellington Management Company against its former portfolio manager Arnold Schneider, claiming breach of his three year non-compete agreement with Wellington, which became one of the most followed and discussed NCC lawsuits in the investment industry. When Mr. Schneider parted ways with Wellington Management Company in December 1996 to start his own firm, three large institutional clients that included pension plans attempted to follow Schneider at his new firm. Wellington brought a lawsuit in the state of Massachusetts against Mr. Schneider, claiming that he violated the non-compete clause in his employment contract. As a result, Mr. Schneider was not allowed by the court to manage money for his former Wellington clients (Sakelaris 1998). This stands in contrast to what happened when Bill Gross, another famous portfolio manager, left PIMCO. Mr. Gross had not signed a NCC with PIMCO since such agreements are not enforceable in the state of California where Gross conducted business for PIMCO (Bansal and Ablan 2014). Therefore, when Mr. Gross resigned from PIMCO and moved to Janus Capital, some clients followed him there, and, unlike in the case of Wellington, there was not much PIMCO could do to stem its clients' migration. Other asset management companies that brought similar lawsuits against their former fund managers that we were able to identify from the business press include Boston Partners Asset

Management, Pilgrim, Baxter & Associates, State Street, Bridgewater Associates, and Citadel Investment Group.[25]

Further information on NCCs reported in the business press comes from coverage of career moves of well-known fund managers. For example, when Michael Price, a well-known fund manager with Franklin Mutual Fund Advisors, left the firm in 2001, it was disclosed that a NCC forbade him from working in the mutual fund business for another 10 years (Wiser 2001). In a more recent description of one such career move we are told that Ryan Caldwell, a portfolio manager for Waddell & Reed "resigned from Waddell & Reed in June 2014, and as soon as his non-compete agreement elapsed, he launched the Chiron Capital Allocation Fund" (Dornbrook 2017). Upon review of such articles, we identified a number of investment companies that at one point had a pending NCC with at least one departing fund manager.[26] These NCCs typically ranged from one to three years and in some cases were accompanied by non-solicitation agreements barring fund managers from doing business with their former firms' clients.

Finally, besides information on NCCs revealed in the business press, textual analysis of SEC filings by mutual fund companies (e.g., Prospectus or Statement of Additional Information) identified a couple of mutual funds self-reporting that their portfolio managers were restricted by NCCs.[27] For example, a 2014 filing by Natixis Funds states that "The non-competition and non-solicitation undertakings will expire the later of one year from the termination of employment, or one year after the period during which severance payments are made pursuant to the agreement."

[25] Lawsuits by these companies are respectively mentioned by Healy (2001), Franecki (1999), Capon (2002), Stevenson and Goldstein (2016), and Herbst-Bayliss (2009). It is likely that some other unreported disputes were settled earlier on out of court and never became public knowledge.

[26] The list includes AIM Fund Management, Boston Company, Boston Partners Asset Management, Bridgewater Associates, Citadel Investment Group, Fidelity Management & Research, Pilgrim, Baxter & Associates, Putnam Investments, State Street, Waddell & Reed, and Wellington Management Company.

[27] See Natixis filing https://www.sec.gov/Archives/edgar/data/1406305/000119312514271200/d755211d485apos.htm.

However, the information from these filings was very scant, revealing primarily whether portfolio manager(s) were subject to a NCC and for how long, without providing any additional information.

Table I. Descriptive statistics

This table reports descriptive statistics. Means are provided for the total sample, the group of treated funds, comprising funds advised from either Texas, Florida or Louisiana, and the control group, comprising all remaining funds. The last two columns provide the difference between the mean value for the treated and for the control group and the corresponding t-statistic. Panel A reports descriptive statistics for fund and family characteristics. Fund size is given by the total net assets under management (AUM) in $ millions. Expense ratio is the annual expense ratio in percent. Turnover ratio is the annual portfolio turnover ratio in percent. Fund age is the age in years. Flow is the percentage growth in net assets under management unrelated to fund performance. Family Size [$ million] measures the total net assets under management aggregated over the fund family in $ millions. Family Size [#managers] is the number of managers employed be the fund family. Family Size [#funds] is the total number of funds run by the family.

	Total Sample	Treated Group	Control Group	Difference	T-stat	
Fund size [$ million]	977.92	902.94	981.90	-78.96	-0.69	
Expense ratio [%/year]	1.40	1.56	1.40	0.16	5.98	***
Turnover ratio [%/year]	95.53	89.24	95.87	-6.63	-1.96	*
Fund age [years]	11.84	13.75	11.73	2.02	3.29	***
Flow [%/year]	39.07	33.68	39.36	-5.67	-0.75	
Family Size [$ million]	23,686	15,488	24,123	-8,636	-6.49	***
Family Size [#managers]	15.63	15.23	15.65	-0.43	-0.51	
Family Size [#funds]	14.27	11.81	14.41	-2.60	-4.20	***

Table II. Impact of changes in NCC enforceability on departure rates

This table presents results from pooled OLS regressions that relate average annual departure rates with changes in NCC enforceability at the state level. The analysis is done at the fund family and yearly level. We calculate the departure rate of a fund company's fund managers in year t as the number of fund manager departures in that year scaled by the number of fund managers in that company. Our main independent variable is Treated · Post, which equals 1 for firms in Florida in 1997–2004, −1 for firms in Texas in 1995–2004 and for firms in Louisiana in 2002–03, and 0 otherwise. Control variables are described in Table I. Regressions are run with family, calendar year, and investment objective fixed effects. T-statistics, based on standard errors clustered at the state level, are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

	Departure Rate	
Treated · Post	-0.0611***	-0.0650***
	(-4.70)	(-4.73)
Expense Ratio		-0.7380
		(-0.93)
Turnover Ratio		0.0031
		(0.82)
Flow		-0.000***
		(-6.33)
Log(Age)		-0.0034
		(-1.18)
Log(TNA)		0.0086
		(1.14)
Observations	2,344	2,344
Within R^2	0.001	0.004

Table III. Impact of changes in NCC enforceability on performance

This table presents results from pooled OLS regressions that relate performance measures with changes in NCC enforceability at the state level. The analysis is done at the fund and month level. Our performance measures include: The raw return (Return), style-adjusted return (Style-adj. Return), DGTW-adjusted return (DGTW), and Carhart 4-factor alpha (Carhart). Style adjusted return (Style-adj. Return) is computed by subtracting from the raw return of a fund the mean raw return of funds with the same investment objective. DGTW-adjusted return is estimated as in Daniel, Grinblatt, Titman, and Wermers (1997), where a stock's characteristic-adjusted return in a given month is computed by subtracting from its return the return of the benchmark portfolio to which that particular stock belongs. These adjusted returns are then value-weighted at the fund portfolio level. Carhart 4-factor alpha is computed for a given fund each month as the difference between the actual return minus the expected return, estimated using factor loadings computed from a regression of the preceding 36 monthly excess returns on the four and five risk factors, respectively. Fund control variables are calculated as in Table I. Our main independent variable is Treated · Post, which equals 1 for firms in Florida in 1997–2004, −1 for firms in Texas in 1995–2004 and for firms in Louisiana in 2002–03, and 0 otherwise. Regressions are run with fund, calendar month, and investment objective fixed effects. T-statistics, based on standard errors clustered at the state level, are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

	Return		Style-adj. Return		DGTW		Carhart	
Treated · Post	0.0023***	0.0014***	0.0023***	0.0016***	0.0013***	0.0007***	0.0018***	0.0015***
	(21.00)	(2.81)	(13.49)	(2.81)	(10.77)	(2.87)	(4.24)	(3.46)
Expense Ratio		-0.0007		0.0066		0.0601*		0.0059
		(-0.02)		(0.16)		(1.77)		(0.09)
Turnover Ratio		0.0004		0.0004		0.0002		0.0002
		(1.35)		(1.33)		(0.40)		(0.56)
Flow		0.0022**		0.0020***		0.0024***		0.0009
		(2.54)		(2.76)		(3.83)		(1.22)
Log(Age)		0.0033***		0.0021***		0.0028***		-0.0009
		(3.96)		(2.76)		(5.95)		(-1.21)
Log(TNA)		-0.0053***		-0.0046***		-0.0025***		-0.0031***
		(-13.49)		(-13.10)		(-14.52)		(-8.63)
Observations	104,043	104,043	104,043	104,043	95,011	95,011	70,656	70,656
Within R^2	0.000	0.008	0.000	0.008	0.000	0.002	0.000	0.004

Table IV. Parallel trends analysis

This table presents results from pooled OLS regressions utilized in Table III that have been augmented with an additional variable to examine performance effects prior to the change in NCC enforceability. Specifically, we augment the regression with a term that interacts an indicator variable for the year prior to the change in NCC enforceability (Pre) with the treatment dummy. Regressions are run with fund, calendar month, and investment objective fixed effects. T-statistics, based on standard errors clustered at the state level, are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

	Return		Style-adj. Return		DGTW		Carhart	
Treated · Pre	-0.0002	-0.0003	-0.0005	-0.0007	0.0002	0.0002	-0.0005	-0.0009
	(-0.12)	(-0.21)	(-0.30)	(-0.43)	(0.09)	(0.13)	(-0.59)	(-1.48)
Treated · Post	0.0022***	0.0012***	0.0021***	0.0013***	0.0013***	0.0008**	0.0016**	0.0010**
	(3.75)	(3.88)	(4.08)	(5.18)	(2.80)	(2.12)	(2.07)	(2.00)
Fund Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	104,043	104,043	104,043	104,043	95,011	95,011	70,656	70,656
Within R^2	0.000	0.008	0.000	0.008	0.000	0.004	0.000	0.001

Table V. Impact of changes in NCC enforceability for each treatment group separately

In this table, we repeat our main analysis of changes in NCC enforceability for each treatment group separately. The construction of dependent variables is described in Table III and control variables are described in Table I. Our main independent variables are Increased · Post, which equals +1 for firms in Florida in 1997–2004 and 0 otherwise as well as Decreased · Post, which equals +1 for firms in Texas in 1995–2004 and 0 otherwise. Regressions are run with fund, calendar month, and investment objective fixed effects. T-statistics, based on standard errors clustered at the state level, are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

	Return		Style-adjusted Return		DGTW		Carhart	
	Florida	Texas	Florida	Texas	Florida	Texas	Florida	Texas
Increased /	0.0009***	-0.0020***	0.0012***	-0.0023***	0.0005**	-0.0008***	0.0013***	-0.0020***
Decreased · Post	(2.90)	(-5.61)	(3.49)	(-7.06)	(2.09)	(-3.45)	(3.84)	(-5.93)
Fund Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	104,043	104,043	104,043	104,043	95,011	95,011	70,656	70,656
Within R^2	0.008	0.008	0.008	0.008	0.002	0.002	0.004	0.004

Table VI. Investments in local companies

This table presents results from pooled OLS regressions that relate performance measures of fund sub-portfolios defined by holdings of local/non-local stocks with changes in NCC enforceability. The analysis is done at the fund and quarter level. A Local and Non-Local Portfolio is constructed for each fund each period and their performance or risk measures are stacked. Local Portfolio includes holdings in the state where the fund advisor is headquartered. All other holdings make up the Non-Local Portfolio. Thus, the analysis is done at the sub-portfolio level and quarterly level. To differentiate between Local and Non-local portfolios, we include an indicator variable Local, which equals one when the sub-portfolio comprises local holdings and zero when it comprises non-local holdings. We interact Local with or main independent variable Treated · Post, which equals 1 for firms in Florida in 1997–2004, −1 for firms in Texas in 1995–2004 and for firms in Louisiana in 2002–03, and 0 otherwise. Our performance measures include the raw return (Return), DGTW-adjusted return (DGTW), and Carhart 4-factor alpha (Carhart), which are constructed as in Table III. Control variables are described in Table I. Regressions are run with fund, calendar quarter, and investment objective fixed effects. T-statistics, based on standard errors clustered at the state level, are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

	Return	DGTW	Carhart
Treated · Post	0.0123***	0.0044**	0.0124***
	(7.61)	(2.39)	(6.71)
Local	0.0011	-0.0008	0.0015
	(0.92)	(-0.36)	(0.89)
Treated · Post · Local	-0.0048	-0.0087	-0.0034
	(-0.96)	(-1.65)	(-1.16)
Fund Controls	Yes	Yes	Yes
Observations	47,583	47,349	47,205
Within R^2	0.007	0.003	0.001

Table VII. Constant manager-fund pairs

In this table, we repeat our main analysis of changes in NCC enforceability using a subsample where the same fund manager was responsible for the fund before and after the change in NCC enforceability. The construction of dependent variables is described in Table III and control variables are described in Table I. Our main independent variable is Treated · Post, which equals 1 for firms in Florida in 1997–2004, −1 for firms in Texas in 1995–2004 and for firms in Louisiana in 2002–03, and 0 otherwise. In Panel A, we utilize the complete sample period. In Panels B, C, and D, we repeat the analysis of Panel A, restricting the period to around three, two, and one years around the change, respectively. Regressions are run with fund, calendar month, and investment objective fixed effects. T-statistics, based on standard errors clustered at the state level, are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Panel A: Complete Sample Period

	Return	Style-adj. Return	DGTW	Carhart
Treated · Post	0.0021***	0.0023***	0.0017**	0.0012***
	(3.08)	(2.80)	(2.13)	(2.70)
Fund Controls	Yes	Yes	Yes	Yes
Observations	101,255	101,255	92,336	68,954
Within R^2	0.008	0.007	0.002	0.004

Panel B: Three years around the change

	Return	Style-adj. Return	DGTW	Carhart
Treated · Post	0.0024***	0.0027***	0.0027***	0.0020***
	(4.59)	(5.87)	(3.04)	(3.05)
Fund Controls	Yes	Yes	Yes	Yes
Observations	100,256	100,256	91,483	68,123
Within R^2	0.008	0.007	0.002	0.004

Panel C: Two years around the change

	Return	Style-adj. Return	DGTW	Carhart
Treated · Post	0.0025***	0.0029***	0.0027***	0.0012**
	(4.81)	(4.34)	(8.04)	(1.97)
Fund Controls	Yes	Yes	Yes	Yes
Observations	99,906	99,906	91,161	67,905
Within R^2	0.008	0.007	0.002	0.004

Panel D: One year around the change

	Return	Style-adj. Return	DGTW	Carhart
Treated · Post	0.0023***	0.0025***	0.0017**	0.0011
	(5.16)	(6.57)	(2.07)	(1.20)
Fund Controls	Yes	Yes	Yes	Yes
Observations	99,615	99,615	90,891	67,676
Within R^2	0.008	0.007	0.002	0.004

Table VIII. Propensity score match

In this table, we repeat our main analysis of changes in NCC enforceability using a subsample of treated and matched untreated funds. To construct the sample of matched untreated funds, we run a logistic regression that relates the probability of a fund being treated to fund characteristics. Using the propensity score from this regression, we determine the nearest untreated neighbor for each treated fund. The construction of dependent variables is described in Table III and control variables are described in Table I. Our main independent variable is Treated · Post, which equals 1 for firms in Florida in 1997–2004, −1 for firms in Texas in 1995–2004 and for firms in Louisiana in 2002–03, and 0 otherwise. Regressions are run with fund, calendar month, and investment objective fixed effects. T-statistics, based on standard errors clustered at the state level, are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

	Return	Style-adj. Return	DGTW	Carhart
Treated · Post	0.0022***	0.0027***	0.0012***	0.0021***
	(6.20)	(9.97)	(3.10)	(4.82)
Fund Controls	Yes	Yes	Yes	Yes
Observations	3,977	3,977	3,714	3,065
Within R^2	0.010	0.010	0.010	0.004

Table IX. Manager skill and NCC impact on fund performance

This table presents results from pooled OLS regressions that relate fund performance measures with changes in NCC enforceability at the state level and their interaction with the skill level of the manager. The analysis is done at the fund and monthly level. The construction of dependent variables is described in Table III and control variables are described in Table I. Our main independent variable is Treated · Post, which equals 1 for firms in Florida in 1997–2004, −1 for firms in Texas in 1995–2004 and for firms in Louisiana in 2002–03, and 0 otherwise. We interact Treated · Post with our skill measure SAT, the average matriculates' SAT score at the institution where the manager obtained her bachelor degree. Regressions are run with fund, calendar month, and investment objective fixed effects. T-statistics, based on standard errors clustered at the state level, are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

	Return	Style-adj. Return	DGTW	Carhart
Treated · Post	0.0133***	0.0158***	0.0118****	0.0225***
	(5.26)	(3.87)	(7.95)	(3.19)
SAT	0.0001	-0.0005	0.0032	-0.0039
	(0.02)	(-0.17)	(1.35)	(-1.18)
Treated · Post · SAT	-0.0113***	-0.0130***	-0.0093***	-0.0169***
	(-5.94)	(-3.81)	(-5.07)	(-3.09)
Fund Controls	Yes	Yes	Yes	Yes
Observations	59,889	59,889	51,551	38,699
Within R^2	0.010	0.009	0.003	0.005

Table X. Size of the internal labor market and NCC impact on fund performance

This table presents results from pooled OLS regressions that relate fund performance measures with changes in NCC enforceability at the state level and their interaction with the size of the internal labor market. The analysis is done at the fund and monthly level. The construction of dependent variables is described in Table III and control variables are described in Table I. Our main independent variable is Treated · Post, which equals 1 for firms in Florida in 1997–2004, −1 for firms in Texas in 1995–2004 and for firms in Louisiana in 2002–03, and 0 otherwise. We interact Treated · Post with SILM, which is given by the total number of funds in the family to which the fund belongs. Regressions are run with fund, calendar month, and investment objective fixed effects. T-statistics, based on standard errors clustered at the state level, are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

	Return	Style-adj. Return	DGTW	Carhart
Treated · Post	0.0004	0.0005*	0.0006**	0.0009**
	(1.34)	(1.73)	(2.33)	(2.58)
SILM	0.0000	0.0000	-0.0000	-0.0000
	(0.57)	(1.00)	(-0.10)	(-0.56)
Treated · Post · SILM	0.0001***	0.0001***	0.0000	0.0001***
	(3.70)	(4.13)	(0.86)	(4.20)
Fund Controls	Yes	Yes	Yes	Yes
Observations	103,308	103,308	94,324	71,184
Within R^2	0.008	0.008	0.002	0.004

Table XI. Impact of changes in NCC enforceability on window dressing

This table presents results from pooled OLS regressions that relate window dressing measures with changes in NCC enforceability at the state level. The analysis is done at the fund and quarter level. Our two window dressing measures are Backwards Holding Return Gap by Solomon, Soltes, and Sosyura (2014) and the Rank Gap by Agarwal, Gay, and Ling (2014). Fund control variables are calculated as in Table I. Our main independent variable is Treated · Post, which equals 1 for firms in Florida in 1997–2004, −1 for firms in Texas in 1995–2004 and for firms in Louisiana in 2002–03, and 0 otherwise. Regressions are run with fund, calendar quarter, and investment objective fixed effects. T-statistics, based on standard errors clustered at the state level, are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

	Backwards Holding Return Gap	Rank Gap
Treated · Post	0.0035***	0.0180***
	(8.02)	(6.89)
Expense Ratio	-0.2661	-0.0447
	(-0.95)	(-0.09)
Turnover Ratio	0.0016**	0.0006
	(2.04)	(0.23)
Flow	0.0010	-0.0011
	(0.53)	(-0.32)
Log(Age)	0.0019	0.0008
	(1.02)	(0.13)
Log(TNA)	0.0019**	0.0174***
	(2.29)	(6.72)
Observations	24,973	24,998
Within R^2	0.002	0.009

Table XII. Impact of changes in NCC enforceability on risk taking

This table presents results from pooled OLS regressions that relate risk taking measures with changes in NCC enforceability at the state level. The analysis is done at the fund and yearly level. Our risk measures of Panel A are Volatility, Semi-Deviation, Downside-Beta following Whaley (2002) and Herding, the holdings value weighted sum of the Lakonishok, Shleifer and Vishny (1992) herding measure. In Panel B, we relate the risk adjustment ratio defined by equation (2) to performance of the first part of the year ($Perf^{First}$), measured as either style-adjusted return or as ranks based on raw returns. Ranks are calculated for each market segment and year separately. They are normalized so that they are equally distributed between zero and one, with the best fund manager in its respective segment getting assigned the rank of one. Fund control variables are calculated as in Table I. Regressions are run with fund, calendar year, and investment objective fixed effects. T-statistics, based on standard errors clustered at the state level, are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Panel A: Portfolio risk and herding

	Volatility	Semi-Deviation	Downside-Beta	Herding
Treated · Post	-0.0015**	-0.0035**	-0.0926**	1.2528***
	(-2.00)	(-2.28)	(-2.24)	(4.54)
Expense Ratio	0.0925	0.1680	1.8942**	5.2176
	(0.62)	(0.58)	(2.15)	(0.21)
Turnover Ratio	0.0004	0.0004	-0.0174**	-0.0151
	(1.41)	(0.56)	(-2.03)	(-0.19)
Flow	0.0000**	0.0000**	0.0000**	0.0040***
	(2.35)	(2.05)	(2.00)	(2.95)
Log(Age)	-0.0018*	-0.0021	-0.0633***	-0.2335
	(-1.69)	(-0.98)	(-3.00)	(-0.92)
Log(TNA)	0.0022***	0.0044***	0.1179***	-0.1211
	(3.27)	(3.31)	(9.72)	(-1.17)
Observations	8,514	8,514	8,514	2,227
Within R^2	0.010	0.008	0.032	0.005

Table XII.-continued

Panel B: Tournament behavior

	Rank	Style-adjusted Return
$Perf^{First}$	-0.0191**	-0.3856***
	(-2.14)	(-3.46)
Treated · Post	0.0041	0.0257*
	(0.26)	(1.82)
$Perf^{First}$ · Treated · Post	0.0512***	0.4394***
	(5.8)	(9.11)
Expense Ratio	0.1828	0.2915
	(0.58)	(0.89)
Turnover Ratio	0.0027	0.0027
	(0.23)	(0.23)
Flow	-0.0025	-0.0019
	(-0.66)	(-0.49)
Log(Age)	0.0187*	0.0171*
	(1.94)	(1.77)
Log(TNA)	-0.0058	-0.0036
	(-1.65)	(-1.05)
Observations	4,244	4,244
Within R^2	0.007	0.014