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What Drives Differences in Management Practices?[†]

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Partnering with the US Census Bureau, we implement a new survey of "structured" management practices in two waves of 35,000 manufacturing plants in 2010 and 2015. We find an enormous dispersion of management practices across plants, with 40 percent of this variation across plants within *the same firm. Management practices account for more than 20 percent of the variation in productivity, a similar, or greater, percentage as that accounted for by R&D, ICT, or human capital. We find evidence of two key drivers to improve management. The business environment, as measured by right-to-work laws, boosts incentive management practices. Learning spillovers, as measured by the arrival of large "Million Dollar Plants" in the county, increases the management scores of incumbents.* (*JEL* D22, D24, L25, L60, M11, M50)

The interest of economists in management goes at least as far back as *On the Sources of Business Profits* by Francis Walker (1887), the founder of the American

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Economic Association and the Superintendent of the 1870 and 1880 Censuses.[1] This interest has persisted until today. For example, Syverson's (2011, p. 336) survey of productivity devotes a section to management as a potential driver, noting that "no potential driver of productivity differences has seen a higher ratio of speculation to actual empirical study." Work evaluating differences in management is often limited to relatively small samples of firms (e.g., Ichniowski, Shaw, and Prennushi 1997), developing countries (e.g., Bloom et al. 2013; Bruhn, Karlan, and Schoar 2018), or particular historical episodes (e.g., Giorcelli 2019). In addition, although previous work on larger samples has measured differences in management across firms and countries, there is no large-scale work on the variations in management between the plants[2] within a firm.

There are compelling theoretical reasons to expect that management matters for performance. Gibbons and Henderson (2013) argue that management practices are a key reason for persistent performance differences across firms due to relational contracts. Brynjolfsson and Milgrom (2013) emphasize the role of complementarities among management and organizational practices. Halac and Prat (2016) show that "engagement traps" can lead to heterogeneity in the adoption of practices even when firms are ex ante identical. By examining the first large sample of plants with information on management practices, this paper provides empirical evidence for the role that these practices play in both firm and plant performance and investigates the causal drivers of why some plants adopt such practices while others do not.

We partnered with the Economic Programs Directorate of the US Census Bureau to develop and conduct the Management and Organizational Practices Survey (MOPS).[3] This is the first-ever mandatory government management survey, covering two separate waves of over 35,000 plants in 2010 and 2015, yielding over 70,000 observations.[4] The sample size, panel structure, high response rate of the survey, its coverage of units within a firm, its links to other Census data, as well as its comprehensive stratified population coverage of industries and geographies makes it unique, and enables us to address some of the major gaps in the recent management literature.

We start by examining whether our management measures are linked to performance. We find that plants using more structured management practices have higher levels of productivity, profitability, growth, survival rates, and innovation. These relationships are robust to a wide range of controls including industry dummies, education, plant, and firm age. The relationship between management practices and performance also holds over time within plants (plants that adopted more of these practices saw improvements in their performance) and across establishments within firms at a point in time (establishments within the same firm with more structured management practices achieve better performance outcomes).

The magnitude of the productivity-management relationship is large. Increasing structured management from the tenth to ninetieth percentile can account for about

[1] Walker was also the second president of MIT and the vice president of the National Academy of Sciences. Arguably Adam Smith's discussion of the Pin Factory and the division of labor was an even earlier antecedent.

[2] Because we are focusing on manufacturing, we use the words "plants" and "establishments" interchangeably.

[3] These survey data are available to qualified researchers on approved projects via the Federal Statistical Research Data Center (FSRDC) network and online in tables (https://www2.census.gov/programs-surveys/mops/tables/2015/mops-survey-tables/mops_survey_tables.pdf) and in aggregated anonymized form (http://managementresearch.com/methodology/).

[4] See the descriptions of MOPS in Buffington et al. (2017) and online Appendix A.

22 percent of the comparable 90–10 spread in productivity. This is about the same as R&D, more than human capital, and almost twice as much as Information and Communication Technologies (ICT). Of course, all of these magnitudes are dependent on a number of other factors, such as the degree of measurement error in each variable, but they do highlight that variation in management practices is likely a key factor accounting for the much-discussed heterogeneity in firm productivity. Technology, human capital, and management are interrelated but distinct: when we examine them jointly, we find they account for about 44 percent of productivity dispersion.

We then turn to examining the variation in management practices across plants, showing three key results. First, there is enormous inter-plant variation in management practices. Although 18 percent of establishments adopt three-quarters or more of a package of basic structured management practices regarding monitoring, targets, and incentives, 27 percent of establishments adopt less than one-half of such practices. Second, about 40 percent of the variation in management practices is across plants *within* the same firm. That is, in multi-plant firms, there is considerable variation in practices across units.[5] The analogy for universities would be that variations in management practices across departments *within* universities are almost equally large as the variations *across* universities. Third, these variations in management practices are increasing in firm size. That is, larger firms have substantially more variation in management practices. This appears to be largely explained by the greater spread of larger firms across different geographies and industries.

We then examine some "drivers" of management practices. We focus our analysis on two main candidates: the business environment (in particular right-to-work laws) and learning spillovers from large plant entry primarily belonging to multinational corporations. We chose these drivers for three reasons. First, we have credible causal identification strategies. Second, they are highly topical with multiple changes in the 2010–2015 time period spanned by our MOPS panel. Third, we show geography plays an important role in shaping variations in management practices. Bloom et al. (2017), the working paper version of this paper, also has analysis of two other drivers: product market competition and education.

On business environment, we exploit two types of quasi-experiments over right-to-work (RTW) laws (Holmes 1998). First, between the two waves of our management panel in 2010 and 2015, two states (Michigan and Indiana) introduced RTW laws in 2012, so this enables us to construct a difference-in-differences (DID) design using contiguous states as comparison groups. We find that RTW rules increase structured management practices around pay, promotion, and dismissals but seem to have little impact on other practices. To demonstrate that our DID design indeed captures the causal effect of RTW on management, we show that there is no evidence for

[5] A literature beginning with Schmalensee (1985) has examined how the variance in *profitability* of business across business divisions decomposes into effects due to company headquarters, industry, and other factors. Several papers have examined productivity differences across sites within a single firm. For example, Chew, Clark, and Bresnahan (1990) looked at 40 operating units in a commercial food division of a large US corporation (the top-ranked unit had revenue-based total factor productivity twice as high as the bottom-ranked); Argote, Beckman, and Epple (1990) showed large differences across 16 Liberty shipyards in World War II; and Blader, Gartenberg, and Pratt (2016) examine productivity differences across sites within a large trucking company. Freeman and Shaw (2009) contains several studies looking at performance differences across the plants of single multinational corporations.

differential pre-trends for the states switching to RTW compared to control states. Furthermore, we use states that switched post-2015 (i.e., outside our data window) to run a placebo analysis, showing again no evidence for changes in management between 2010 and 2015 for these placebo states. In our second approach, we implement a spatial regression discontinuity (RD) design where we use distance to the border as a running variable and crossing the border as our discontinuity threshold. The results from the RD design are very similar to the ones we find in the DID.

To investigate learning spillovers, we build on Greenstone, Hornbeck, and Moretti's (2010) identification strategy using "Million Dollar Plants" (MDPs), large investments for which both a winning county and a runner-up county are known. Comparing the counties that "won" the large, typically multinational plant versus the county that narrowly "lost," we find a significant positive impact on the management practices of incumbent plants in the county. Importantly, the positive spillovers only arise if the plant is in an industry where there are frequent flows in managerial labor from the MDP's industry, suggesting that the movement of managers is a mechanism through which learning occurs. We also show positive impacts on jobs and productivity.

The existing management and productivity literature is motivated by a number of different theoretical perspectives (e.g., Penrose 1959, Syverson 2011, Gibbons and Roberts 2013). One perspective that binds our drivers together follows Walker (1887) and considers some forms of structured management practices to be akin to a productivity-enhancing technology. This naturally raises the question of why all plants do not immediately adopt these practices. One factor is information: not all firms are aware of the practices or believe that they would be beneficial. This motivates our examination of diffusion-based learning and informational spillovers from Million Dollar Plants. Another factor is institutional constraints such as union power: this motivates our examination of regulation, in particular right-to-work laws. Of course, there are many other factors that can influence structured management, and we hope that the data we have generated and made available will help future researchers isolate other drivers.

Our paper also builds on a rich empirical literature on the effects of management and organizational practices on performance. One group of papers uses cross-sectional or occasionally panel data on management (or organizational) practices and firm performance. Examples of this would include Black and Lynch (2001, 2004); Bresnahan, Brynjolfsson, and Hitt (2002); Brynjolfsson, Hitt, and Yang (2002); Cappelli and Neumark (2001); Easton and Jarrell (1998); Huselid (1995); Huselid and Becker (1996); Ichniowski and Shaw (1999); and Osterman (1994). These studies tend to find positive associations in the cross sections, but they tend to disappear in the panel (see the survey by Bloom and Van Reenen 2011). The sample response rates are also usually low (at least compared with the MOPS) and the frames usually tilted toward very large firms. Another group of studies focuses on smaller numbers of firms sometimes even looking across sites in a single firm (labeled "insider econometrics" by Bartel, Ichniowski, and Shaw 2004). Examples would include Bartel, Ichniowski, and Shaw (2007); Bandiera, Barankay, and Rasul (2005, 2007); Griffith and Neely (2009); Hamilton, Nickerson, and Owan (2003); Ichniowski, Shaw, and Prennushi (1997); and Lazear (2000). These tend to focus on specific forms of management

practices such as incentive pay. Much has been learned from these studies, but because samples are small, it is difficult to generalize across larger swathes of the economy.

The paper is structured as follows. In Section I, we describe the management survey. In Section II, we outline the relationship between management and performance. In Section III we detail the variation of management practices across and between firms. And in Section IV, we examine potential drivers of management practices. Finally, in Section V we conclude and highlight areas for future analysis. Online Appendices go into more detail on data (A), theory (B), and a comparison with the World Management Survey (C).

I. Management and Organizational Practices Survey

The Management and Organizational Practices Survey (MOPS) was jointly funded by the Census Bureau, the National Science Foundation, the MIT Initiative on the Digital Economy, the Sloan Foundation, and the Kauffman Foundation. It was fielded in 2011 and 2016 as a supplement to the 2010 and 2015 Annual Survey of Manufactures (ASM), with response required by law.[6] The original design was based in part on a survey tool used by the World Bank and adapted to the United States through two years of development and cognitive testing by the Census Bureau.[7] It was sent electronically as well as by mail to the ASM respondent for each establishment, which was typically the plant manager, financial controller, CEO, CFO, or general manager (see online Appendix Table A1 for details). Most respondents (58 percent in 2010 and 80 percent in 2015) completed the survey electronically, with the remainder completing the survey by paper. Non-respondents were mailed a follow-up letter after six weeks if no response had been received. A second follow-up letter was mailed if no response had been received after 12 weeks. The first follow-up letter included a copy of the MOPS instrument. An administrative error occurred in 2010 when merging electronic and paper collection data that caused some respondents to receive the first follow-up even though they had already responded, and as a result, in some cases there were two different sets of respondents for the same plant. We exploit this accident to deal with measurement error in the management scores in Section II.

A. *Measuring Management*

The survey in both waves contained 16 management questions in three main areas: monitoring, targets, and incentives, based on the World Management Survey (WMS) of Bloom and Van Reenen (2007). This was itself based in part on the principles of continuous monitoring, evaluation and improvement from Lean manufacturing (e.g., Womack, Jones, and Roos 1990).[8] The survey also contained questions on other organizational practices (such as decentralization) based on work by

[6] For more details, see Buffington et al. (2017). Note that MOPS surveys for calendar year X are sent in spring of year $X + 1$ to collect retrospective data.

[7] See Buffington, Herrell, and Ohlmacher (2016) for more information on the testing and development of the MOPS.

[8] The 16 questions which are the main focus of this paper did not change over the two waves of the MOPS.

Bresnahan, Brynjolfsson, and Hitt (2002) as well as some background questions on the plant and the respondent.[9]

The monitoring section asked firms about their collection and use of information to monitor and improve the production process. For example, the survey asked, "How frequently were performance indicators tracked at the establishment?," with response options ranging from "never" to "hourly or more frequently." The targets section asked about the design, integration, and realism of production targets. For example, the survey asked, "What was the time-frame of production targets?," with answers ranging from "no production targets" to "combination of short-term and long-term production targets." Finally, the incentives section asked about non-managerial and managerial bonus, promotion, and reassignment/dismissal practices. For example, the survey asked, "How were managers promoted at the establishment?," with answers ranging from "mainly on factors other than performance and ability, for example tenure or family connections" to "solely on performance and ability." [10]

In our analysis, we aggregate the results from these 16 questions into a single measure which we call "structured management." This management score is the unweighted average of the score for each of the 16 questions, where the responses to each question are first scored to be on a 0–1 scale. Thus, the summary measure is scaled from 0 to 1, with 0 representing an establishment that selected the category which received the lowest score (little structure around performance monitoring, targets, and incentives) on all 16 management dimensions and 1 representing an establishment that selected the category that received the highest score (an explicit structured focus on performance monitoring, detailed targets, and strong performance incentives) on all 16 dimensions (see more details in online Appendix A and online Appendix Table A2).

Figure 1 plots the histogram of plant management scores for the 2010 wave, which displays enormous dispersion.[11] While 18 percent of establishments have a management score of at least 0.75, meaning they adopt 75 percent of the most structured management practices, 27 percent of establishments receive a score of less than 0.5 (that is, they adopt less than one-half of the practices).

Finally, our data collection includes recall questions (in 2015 asking about 2010 and in 2010 asking about 2005). This allows us to construct recall measures for the management score in 2005, and for missing observations in 2010. By comparing the actual management scores in 2010 to the 2010 recall values from the 2015 survey, we can also benchmark the quality of recall responses. Not surprisingly, we find that a key variable that determines the quality of recall management score is the tenure at the establishment of the manager responding to the survey: if the respondent's tenure started at least one year before the period of the recall, response quality is

[9] The 2015 MOPS survey wave also included questions on two new content areas, "Data and Decision Making" and "Uncertainty." See Buffington et al. (2017) for more information on the differences in content between survey waves of the MOPS. MOPS has since been used in several other countries with various modifications: see http://managementresearch.com/. It has been used by at least one company: see https://www.census.gov/newsroom/blogs/research-matters/2019/02/fresh_uses_for_them.html.

[10] The full questionnaire is available at: https://www.census.gov/programs-surveys/mops/technical-documentation/questionnaires.html.

[11] The average management score over the entire sample is 0.615 (see online Appendix Table A4). We test and find that (controlling for recall dummy) management score is marginally (0.013) higher in 2015 compared to 2010.

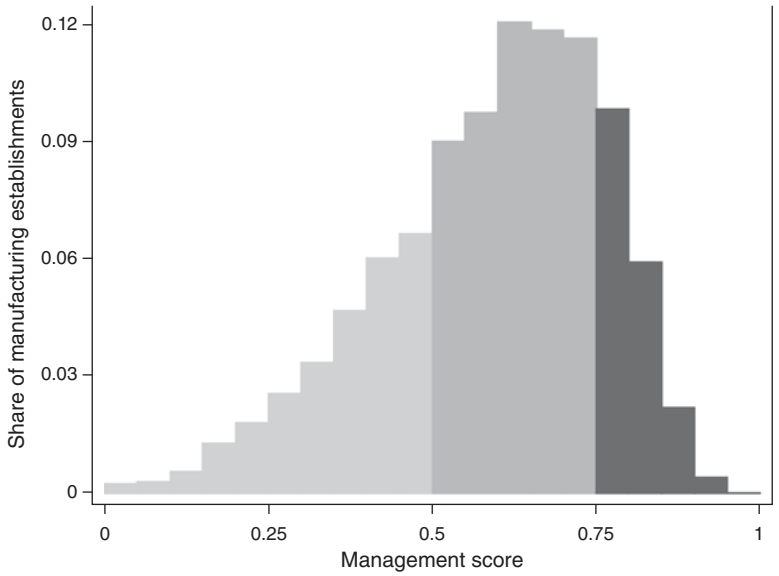

Figure 1. The Wide Spread of Management Scores across Establishments

Notes: The management score is the unweighted average of the score for each of the 16 questions, where each question is first normalized to be on a 0–1 scale. The sample is all 2010 MOPS observations with at least 11 non-missing responses to management questions and a successful match to ASM, which were also included in ASM tabulations, and have positive value added, positive employment, and positive imputed capital in the ASM. Figure is weighted using ASM weights.

high.[12] As a result of this benchmarking exercise, we only use 2005 and 2010 recall values for the management score when the survey respondent has at least seven years of tenure at the establishment. We also include a "recall dummy" in regressions to control for the fact that some observations are using recall data.

B. *Sample and Sample Selection*

The sampling frames for the 2010 and 2015 MOPS were the 2010 and 2015 ASM respectively, which included around 50,000 plants in each wave.[13] The response rate for the first survey wave was approximately 78 percent, and the response rate for the second survey wave was approximately 74 percent.[14] For most of our analysis, for each wave we further restrict the sample to establishments with at least ten non-missing responses to management questions that also have positive value added

[12] For 2015 managers answering 2010 questions, if the respondent started at the establishments in 2008 or earlier, the correlation between recall and actual 2010 management scores is 0.48. As discussed below, the correlation between management scores collected from two managers in the *same* plant at the *same* time is 0.55: close to the recall correlation for managers with long tenure, suggesting high recall fidelity.

[13] Note that sample counts have been rounded for disclosure reasons throughout the paper.

[14] The Census Bureau also constructs a measure of response coverage called the Unit Response Rate (URR). The URR is measured as the number of establishments included in the published tables divided by the number of establishments in the mail sample less those establishments for which there is evidence that they are inactive or out-of-scope. The URR for the 2015 survey wave was approximately 71 percent. See https://www.census.gov/programs-surveys/mops/technical-documentation/methodology.html for more information on the URR.

and positive employment and for which we were able to impute a capital measure. Online Appendix Table A3 shows how our various samples are derived from the universe of establishments.

Online Appendix Table A4 provides more descriptive statistics. The mean establishment size is 177 employees and the median (fuzzed) is 86. The average establishment in our sample has been in operation for 21 years,[15] 44 percent of managers and 9.8 percent of non-managers have college degrees, 12.2 percent of workers are in unions, and 67.9 percent of plants are part of larger multi-plant firms. Finally, online Appendix Table A5 reports the results for linear probability models for the different steps in the sampling process for the 2010 MOPS wave. We show that establishments that were mailed and responded to the MOPS survey are somewhat larger and more productive compared to those that did not respond, but these differences are quantitatively small.

C. *Performance Measures*

In addition to our management data, we also use data from other Census and non-Census datasets to create our measures of performance. We use establishment-level data on sales, value-added, and labor inputs from the ASM to create measures of growth and labor productivity. As described in detail in online Appendix A, we also combine capital stock data from the Census of Manufactures (CM) with investment data from the ASM and apply the Perpetual Inventory Method to construct capital stock at the establishment level, which we use to create measures of total factor productivity.[16] For innovation, we use firm-level data from the 2010 Business R&D and Innovation Survey (BRDIS) on R&D expenditure and patent applications by the establishment's parent firm from the USPTO.

II. Management and Performance

Given the variations in management practices noted above, an immediate question is whether these practices link to performance outcomes. In this section, we investigate whether these more structured management practices are correlated with five alternative measures of performance (productivity, profitability, innovation, survival, and growth). Although there is good reason to think management practices affect performance from both theory and extensive case literature, we do not necessarily attribute a causal interpretation to the results in this section. Instead, it suffices to think about these results as a way to establish whether this management survey is systematically capturing meaningful content rather than just statistical noise.

[15] Measured age is defined as the number of years the establishment has been alive in the Longitudinal Business Database (LBD), starting from its first year in 1976. Hence, age is truncated at 30 years in 2005, and we keep the same truncation for 2010 and 2015 for comparability over years.

[16] We use TFP as shorthand for revenue-based Total Factor Productivity (TFPR). This will contain an element of the mark-up (see Foster, Haltiwanger, and Syverson 2008 and Hsieh and Klenow 2009) but is likely to be correlated with quantity-based TFP (see Bartelsman, Haltiwanger, and Scarpetta 2013). For a detailed discussion about micro-level measures of TFP, see Foster et al. (2017).

A. *Management and Productivity*

We start by looking at the relation between labor productivity and management. Suppose that the establishment production function is

$$Y_{it} = A_{it} K_{it}^{\alpha} L_{it}^{\beta} I_{it}^{\gamma} e^{\delta M_{it}} e^{\mu X_{it}}, \tag{1}$$

where Y_{it} is output (shipments deflated by NAICS six-digit price deflator), A_{it} is (total factor) productivity (excluding management practices), K_{it} denotes the establishment's capital stock at the beginning of the period, L_{it} are labor inputs, I_{it} are intermediate inputs (materials plus energy), X_{it} is a vector of additional factors such as education, and M_{it} is our management score.[17] Management is an inherently multidimensional concept, but for this study we focus on a single dimension: the extent to which firms adopt more structured practices.[18]

Dividing by labor and taking logs we can rewrite this in a form to estimate on the data:

$$\log\left(\frac{Y_{it}}{L_{it}}\right) = \alpha \log\left(\frac{K_{it}}{L_{it}}\right) + \gamma \log\left(\frac{I_{it}}{L_{it}}\right) + (\alpha + \beta + \gamma - 1)\log(L_{it}) + \delta M_{it} + \mu X_{it} + f_i + \tau_t + u_{it}, \tag{2}$$

where we have substituted the productivity term (A_{it}) for a set of industry (or firm or establishment) fixed effects f_i, time dummies τ_t, and a stochastic residual u_{it}. Because we may have multiple establishments per firm, we also cluster our standard errors at the firm level.

In column 1 of Table 1, we start by running a basic regression of labor productivity (measured as log(output/employee)) on our management score without any controls other than year and recall dummies. The sample pools responses from 2015 and 2010 and the recall information for 2005 and 2010 (asked in 2010 and 2015 respectively). We find a highly significant coefficient of 1.351, suggesting that every 10 percentage point increase in our management score is associated with a 14.5 percent ($= \exp(0.1351) - 1$) increase in labor productivity. To get a sense of this magnitude, our management score has a sample mean of 0.615 and a standard deviation of 0.172 (see the sample statistics in online Appendix Table A4), so that a one standard deviation change in management is associated with a 26.2 percent ($= \exp(0.172 \times 1.351)$) higher level of labor productivity. We provide more detailed analysis of magnitudes in Section IIE. In column 2 of Table 1, we estimate the full specification from equation (1) with capital, intermediates, labor, employee

[17] We put the management score and X_{it} controls to the exponential simply so that after taking logs we can include them in levels rather than logs.

[18] The individual practices are highly correlated, which may reflect either a common underlying driver or complementarities among the practices (Brynjolfsson and Milgrom 2013). In this exercise, we use the mean of the share of practices adopted, but other measures like the principal factor component or z-score yield very similar results. Indeed, we show in online Appendix Table A6 that key results in this section hold when we use every management question individually instead of an overall index.

Table 1—Plant Management Scores and Performance

	log(output/employment)					Profit/sales		log(output/emp)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Management	1.351	0.209	0.079	0.096	0.074	0.095	0.051	0.105
	(0.039)	(0.013)	(0.030)	(0.025)	(0.025)	(0.005)	(0.010)	(0.045)
log(capital/emp)		0.100	0.012	0.096	0.096	0.026	0.023	0.004
		(0.003)	(0.010)	(0.005)	(0.005)	(0.001)	(0.002)	(0.017)
log(material/emp)		0.495	0.333	0.525	0.534	−0.068	−0.069	0.309
		(0.004)	(0.016)	(0.008)	(0.009)	(0.001)	(0.003)	(0.030)
log(employment)		−0.027	−0.192	−0.054	−0.053	−0.002	−0.009	−0.217
		(0.002)	(0.019)	(0.005)	(0.005)	(0.001)	(0.002)	(0.034)
Share employees w/ a college degree		0.223	0.013	0.180	0.179	0.023	0.025	0.064
		(0.015)	(0.031)	(0.024)	(0.024)	(0.007)	(0.011)	(0.066)
Observations	~82,500	~82,500	~33,000	~43,000	~43,000	~82,500	~43,000	~10,000
Num. establishments	~52,500	~52,500	~16,500	~26,500	~26,500	~52,500	~26,500	~5,000
Num. firms (clusters)	~32,500	~32,500	~9,800	~5,100	~5,100	~32,500	~5,100	~4,200
Sample	All	All	Panel	Multi-plant firm		All	Multi-plant	Panel-Same responder
Fixed effects	None	Industry	Establish.	Firm	Firm × year	Industry	Firm × year	Establish.

Notes: OLS coefficients with standard errors in parentheses (clustered at the firm level). The management score is the unweighted average of the score for each of the 16 questions, where each question is first normalized to be on a 0–1 scale. The sample in columns 1, 2, and 6 is all MOPS observations with at least 10 non-missing responses to management questions and a successful match to ASM, which were also included in ASM tabulations, have positive value added, positive employment, and positive imputed capital in the ASM. Recalls are used for respondents with at least seven years of tenure at the establishment. Sample in column 3 includes only establishments with 2 observations (in 2010 and 2015 excluding recalls). Sample in columns 4, 5, and 7 includes establishments that have at least 1 sibling (i.e., from the same parent firm) in MOPS within the year. Column 8 reports results for the sample in column 3 restricted to establishments with same respondent for 2010 and 2015. In columns 1 through 5 and 8, the dependent variable is log(real output over total employment). In column 6 and 7, profits are measured by value added minus wages and salaries over total value of shipments. All regressions include year fixed effect and recall dummy.

education, and industry dummies on the right-hand side. This reduces the coefficient on management to 0.209.

Even after conditioning on many observables, a key question that remains is whether our estimated ordinary least squares (OLS) management coefficient captures a relation between management and productivity, or whether it is just correlated with omitted factors that affect the management score and the productivity measure. To address this, we focus on plants which were in the 2010 and 2015 panel, drop all recall data, and estimate models including plant fixed effects in order to, at least partially, address this concern over omitted factors.[19] As long as the unobserved factors that are correlated with management are fixed over time at the establishment level (corresponding to f_i in equation (2)), we can difference them out by running a fixed effect panel regression. Column 3 reports the results for the 2010–2015 pooled panel regression (including a 2015 time dummy). The

[19] The sample is smaller because we drop 2005, and also because it conditions on establishments where we have data on management (and other factors) in both 2010 and 2015. This means we have to drop plants that entered or exited after 2010, and plants that were not part of the ASM rotating panel.

coefficient on management, 0.079, remains significant at the 1 percent level. Of course, this coefficient may still be upwardly biased if management practices are proxies for time-varying unobserved productivity shocks. These could include firm-specific changes in leadership styles, culture, or other factors that also happen to be correlated with the management practices that we measure, and our results should be interpreted accordingly. On the other hand, the coefficient on management could also be attenuated toward zero by measurement error, and this downward bias is likely to become much worse in the fixed effect specification.[20]

The rich structure of our data also allows us to compare firm-level versus establishment-level management practices. In particular, by restricting our analysis to multi-establishment firms, we can check whether there is a correlation between structured management and productivity *within* a firm. Column 4 of Table 1 shows OLS estimates for the subsample of multi-establishment firms with firm fixed effects included. The management coefficient of 0.096 is highly significant. In this column, the coefficient on management is identified partially off the variation of management and productivity across plants within each firm in a given year, but also from the time series variation of plants across firms within the panel. To use solely the first source of variation we also include firm-by-year dummies in column 5 which leads to a management coefficient of 0.074. Hence, even within the very same firm, when management practices differ across establishments, we find large differences in productivity associated with variations in management practices.[21] This is reassuring, since we will show in Section III that there is a large amount of management variation across plants within the same firm.

How do these estimates compare with earlier results? The easiest way to make the comparison is to consider the association between TFP and a one standard deviation change in the management index. Call this δ_M. Using column 2 of Table 1, we have a coefficient of 0.209 and a standard deviation of the management score of 0.172. Therefore, $\delta_M = 0.036$. In the Bloom and Van Reenen (2007) study using WMS data, equivalent estimates from column 4 of their Table 1 is 0.040, which is $\delta_M = 0.040$ (their management measures are already z-scored to be in standard deviation units).[22] So these associations seem broadly comparable between the two datasets. Online Appendix C gives a detailed comparison of two methods of collecting management data in the MOPS and WMS and show a strong correlation between the two measures where we have overlapping firms.

Firms care more about profits rather than productivity per se, so we use the operating profits to sales ratio as an alternative measure of firm performance in the next two columns of Table 1. Column 6 has the same specification as column 2 except

[20] There is certainly evidence of this from the coefficient on capital, which falls dramatically when establishment fixed effects are added, which is a common result in the literature.

[21] Running regressions in the cross section with firm fixed effects is an even more general model as we (i) allow the coefficients on the factor inputs (and other controls) to be year-specific and (ii) we switch off the time series variation of plant-specific productivity and management within a firm. When running cross-section regressions with firm dummies separately in each MOPS wave, we obtain significant coefficients on management in each year of a similar magnitude to the pooled estimate in column 5. This shows that the variation from (i) and (ii) are not contributing much to the identification of the management coefficient in column 5.

[22] In the firm-level version of the MOPS data, the coefficient on management is 0.307 (from online Appendix Table A10) and the standard deviation is 0.16. This implies $\beta_M = 0.307 \times 0.16 = 0.049$, slightly higher than the Bloom and Van Reenen (2007) estimates.

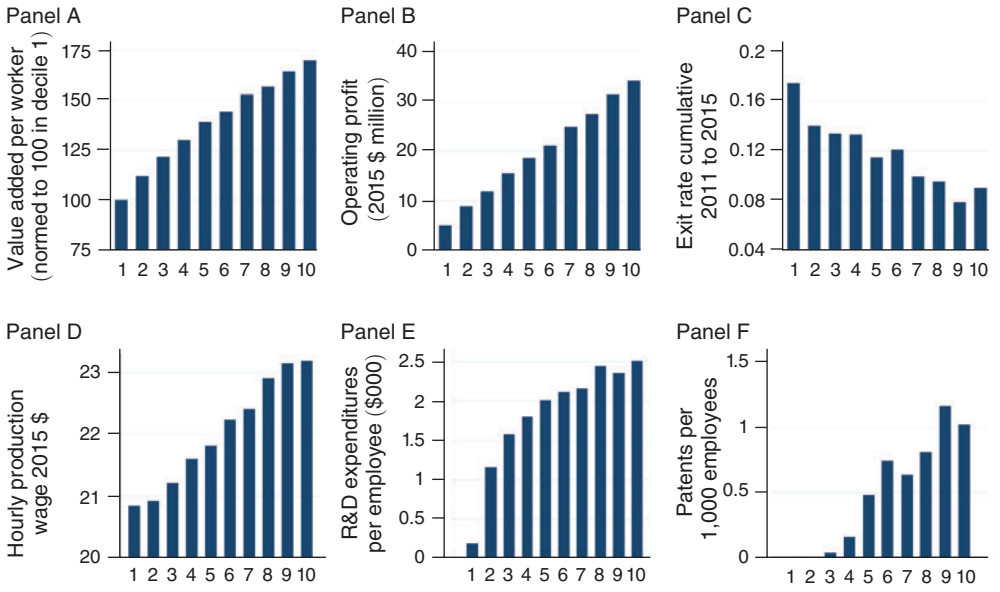

FIGURE 2. PERFORMANCE AND STRUCTURED MANAGEMENT

Notes: The management score is the unweighted average of the score for each of the 16 questions, where each question is first normalized to be on a 0–1 scale. The sample in panels A, B, and F is all MOPS observations with at least 10 non-missing responses to management questions and a successful match to ASM, which were also included in ASM tabulations, have positive value added, positive employment, and positive imputed capital in the ASM. The sample in panels C, E, and F only uses observations where we have 2010 MOPS management data. "Exit rate" indicates whether a plant alive in 2011 had exited the economy by 2015. In panels E and F, we also condition on non-missing R&D or patents requests count in the BRDIS survey. Management deciles are recalculated for the different samples. The figures are unweighted.

with profits as the dependent variable and column 7 mimics column 5 including firm by time dummies. We observe a significant management coefficient in both of these specifications. Figure 2 shows that in the raw data we observe a positive correlation with productivity and profits, and also with measures of innovation such as patents and R&D,[23] as well as with hourly production wages.

One of the issues of concern is whether plant managers "talk up" their management practices regardless of the underlying reality. If this bias is stable over time, then by including plant fixed effects we control for this potential bias. But it could be that the bias changes over time. One way that this would be revealed would be by comparing across different respondents. Were this to be a first order concern, the productivity-management relationship might be different when a different manager answered the survey in 2015 than in 2010 compared to when the same manager answered the survey in both years. Column 8 of Table 1 reports results for when the survey was answered by the same individual respondent in 2010 and 2015, revealing a similar coefficient.

[23] See, for example, Henderson and Cockburn (1994) for a model linking managerial competence and innovation.

B. *Cross-Industry Heterogeneity in the Performance-Management Relationship*

So far, we have established a strong correlation between labor productivity and the adoption of management practices. It is likely that this relation is somewhat contingent on the firm's environment, and that the adoption of particular management practices is more important in some contexts than in others. To investigate this heterogeneity, we estimate the specification in column 2 of Table 1 for the 86 4-digit manufacturing NAICS categories. Online Appendix Figure A1 plots the smoothed histogram of the 86 regression coefficients.[24] To avoid over estimating the dispersion in management coefficients, we apply an Empirical Bayes Shrinkage procedure.[25] The distribution is centered on 0.2, which reassuringly is the coefficient from the pooled regression. All establishments operate in industries with a positive labor productivity-management relation. There is indeed a lot of heterogeneity between sectors, and an F-test for the null of no difference across industries is easily rejected (p-value < 0.001). These findings suggest that the importance of structured management varies across environments, as one would expect.

We leave a more thorough investigation of the reasons for this heterogeneity for future research, but we did examine whether structured management was less important for productivity in sectors where innovation mattered a lot (e.g., high industry intensities of R&D and/or patenting), as perhaps an over-focus on productive efficiency could dull creativity. Interestingly, we found that the productivity-management relationship was actually *stronger* in these high tech industries, perhaps implying that rigorous management is as important in R&D labs as it is in production plants.

C. *Measurement Error*

Before turning to additional performance outcomes, we take a moment here to discuss concerns about measurement error. Estimates in Bloom and Van Reenen (2007) from independent repeat management surveys (at the same point of time) imply that measurement error accounts for about one-half of the variation in management score, making this an important issue. Including establishment fixed effects controls for measurement error in the management score if it is plant-specific and fixed over time. But we can go further in characterizing measurement error by exploiting a valuable feature of the 2010 MOPS survey, which is that approximately 500 plants from our baseline sample have two surveys filled out by different respondents. That is, for this set of plants, two individuals (for example, "John Doe" the plant manager and "Jane Smith" the financial controller) both independently filled out the MOPS survey. This is most likely because a follow-up letter was mailed to a random set of plants in error that included a form and online login information, and an individual other than the original respondent received the letter. We confirm this measurement also turns out to be independent of any firm- or plant-level observable characteristics such as employment, productivity, or the number of plants in the firm (see online Appendix Table A7), and thus appears to be effectively white noise. These double

[24] To comply with Census disclosure avoidance requirements, we do not report the actual coefficients industry by industry, but a smoothed histogram.

[25] We follow closely Chandra et al. (2016).

TABLE 2—MANAGEMENT AND PERFORMANCE, ACCOUNTING FOR MEASUREMENT ERROR

	Baseline OLS (1)	Duplicates sample OLS (2)	Duplicates sample IV (3)	Implied share measurement error (4)
Dependent variable: log(output)	4.264	4.465	9.174	0.513
	(0.057)	(0.398)	(1.073)	
Dependent variable: log(employment)	2.913	3.401	6.949	0.511
	(0.044)	(0.348)	(0.890)	
Dependent variable: log(output/employment)	1.351	1.094	2.344	0.533
	(0.039)	(0.266)	(0.563)	
Dependent variable: log(output/employment) Deviations from industry mean	0.535	0.549	1.104	0.503
	(0.02)	(0.201)	(0.389)	
Observations	~82,500	~500	~500	

Notes: Each row reports the results from regressions on a different dependent variable listed in the left column. Columns 1 to 3 report regression coefficients with standard errors in parentheses (clustered at the firm level) for regressions ran on three different specifications. Column 1 reports results from OLS regressions for the baseline sample (as in columns 1 and 2 of Table 1). Columns 2 and 3 report results from OLS and IV regressions for the sample with duplicate reports. In column 3, each management score is instrumented using the duplicate report. Regressions in columns 1 include year fixed effects and a recall dummy.

responses are extremely valuable in enabling an accurate gauge of survey measurement error, because within a three-month window we have two measures of the same plant-level management score provided by two separate respondents.

First, we use these duplicate responses to estimate the degree of measurement error by correlation analysis. Assuming that the two responses have independent measurement error with standard deviation σ_e^2, and defining σ_m^2 as the true management standard deviation, the correlation between the two surveys will be $\sigma_m^2/(\sigma_m^2 + \sigma_e^2)$, and the measurement error share will be $\sigma_e^2/(\sigma_m^2 + \sigma_e^2) = 0.454$, where $\left(\sigma_m^2 + \sigma_e^2\right)$ is the variance of the observed management score on 500 double score sample and σ_e^2 is one-half of the variance of the difference between the first and second management score. Interestingly, this 45 percent share of the variation from measurement error is very similar to the 49 percent value obtained in the World Management Survey from second independent telephone interviews (Bloom and Van Reenen 2007).

Second, we use these duplicates to instrument one management score with the other to overcome attenuation bias in our OLS performance estimates. We perform this analysis in Table 2, starting by analyzing output in the first row. First, in column 1 we regress log(output) on management for the entire sample. Then in column 2 we re-run this estimate on the 500 duplicates finding a very similar estimation coefficient, suggesting this duplicate sample is similar to the whole sample. Column 3 is the key specification in which we instrument the first management score with its second duplicate score, finding that the point estimate roughly doubles from 4.465 to 9.174. In column 4 we compare these OLS and IV coefficients to estimate that measurement error accounts for about 51 percent of the management variation. We repeat this exercise for log(employment) in the second row, for log(output/employee) in the third row (replicating column 1 of Table 1), and for

Table 3—Management, Exit, and Growth

Time window	2010 to 2015 (1)	2014 to 2015 (2)	2010 to 2015 (3)	2010 to 2015 (4)	2010 to 2015 (5)
Panel A. Dependent variable: exit					
Management	−0.180	−0.035	−0.286	−0.153	−0.280
	(0.014)	(0.007)	(0.033)	(0.014)	(0.033)
log(value added/emp)				−0.025	−0.039
				(0.003)	(0.006)
Marginal R^2 for management^				0.506	0.665
Marginal R^2 for log(value added/emp)^				0.308	0.482
Panel B. Dependent variable: employment growth					
Management	0.412	0.088	0.629	0.326	0.609
	(0.033)	(0.018)	(0.075)	(0.035)	(0.075)
log(value added/emp)				0.078	0.131
				(0.007)	(0.013)
Marginal R^2 for management^				0.394	0.535
Marginal R^2 for log(value added/emp)^				0.525	0.915
Firm fixed effects	No	No	Yes	No	Yes
Observations	∼32,000	∼29,000	∼17,000	∼32,000	∼17,000

Notes: OLS coefficients with standard errors in parentheses (clustered at the firm level). The management score is the unweighted average of the score for each of the 16 questions, where each question is first normalized to be on a 0–1 scale. The sample in column 1 and 4 is all MOPS observations with valid management score in 2010 and a successful match to ASM, which were also included in ASM tabulations, have positive value added, positive employment, and positive imputed capital in the ASM. In column 2, we use the same sample but also condition on survival up to 2014. In columns 3 and 5, we use the 2010 sample from column 1 but also condition on the establishment having a sibling in the sample (i.e., same parent firm). In panel A, the dependent variable is a dummy that takes the value of 1 for exit between the two years listed in the *Time window* row. In panel B, the dependent variable is employment growth between the two years specified in the *Time window* row. Growth between years s and t is calculated as $2 \times (L_t - L_s)/(L_t + L_s)$ following Davis and Haltiwanger (1992). ^ denotes R^2 scaled up by 100 to show enough significant figures.

industry normalized log(output/employee) in the fourth row. These produce qualitatively similar results to the first row: (i) the 500 establishment duplicate sample yields a similar coefficient on management to the whole sample; and (ii) the IV estimates are roughly twice the OLS estimates (similar to the 45 percent estimate of measurement error from the 2 management score variances and covariance noted above). These results imply that about half the variation in the management data is measurement error.

D. *Management Practices, Survival, and Growth*

In Table 3, we focus on two other important outcomes: survival or its flipside, exit (panel A) and employment growth (panel B). Because the Census tracks the survival and employment of all plants in the Longitudinal Business Database (LBD), we have up to five years of data on the MOPS 2010 cohort (2015 is the last year where we have reliable data at time of writing).[26] In column 1 we examine whether establishments have exited the economy by the end of December 2015. The coefficient

[26] As additional years of the Longitudinal Business Database (LBD) become available, we can examine increasingly long-run relationships between management practices, employment, and survival.

is large and highly significant (−0.180). This indicates that a one standard deviation increase in the management score (0.172) is associated with a 3.1 percentage point reduction in the probability of establishment death, which is 26 percent of the mean death rate of 11.8 percent.[27] In column 2, we test if the 2010 management score can predict the exit rates 5 years later between 2014 and 2015, and find that it can. This highlights how the management score has significant predictive power for longer-run as well as shorter-run plant performance.

In column 3 of Table 3, we include firm effects in the *Exit by 2015* equation of column 1. We still observe a negative and significant coefficient, showing that even within the same firm, a plant with a relatively low management score is relatively more likely to be closed down. Interestingly, this coefficient is even larger than in column 1. A possible interpretation is that for a single plant firm, it is the market signal of negative profits that should induce exit. In contrast, for a multi-plant firm, the headquarters is deciding which plants to shut down and this might be easier to accomplish (e.g., by moving assets and employment from one plant to another). Hence, such creative destruction may be more easily implemented within firms than between them.[28]

In column 4 of Table 3, we include 2010 labor productivity (value added per worker) into the specification of column 1 and then add firm by year fixed effects in column 5. Less-productive plants are more likely to exit, but the coefficient on management practices is robust to this and remains significant. Since management practices and productivity are correlated, the coefficient on management practices falls. For example, in column 4 it is −0.153 compared to −0.180 in column 1. Strikingly, the contribution of management practices in accounting for exit is *larger* than productivity (e.g., a marginal R^2 of 0.005 for management practices compared to 0.003 for productivity in column 4).

In panel B of Table 3, we repeat the specifications of panel A using employment growth as the outcome. [29] The findings here mirror the exit analysis with firms who had higher management scores in 2010 being significantly more likely to grow over the next five years. Using the results from column 1, a one standard deviation increase in management practices is associated with 7 percent faster growth.

One interesting extension we ran on Table 3 is to examine if the association between management practices and plant performance varied with plant age. In short (details in online Appendix Table A8) the management score was much more strongly related to growth and survival for younger plants: for example, the exit relationship was twice as strong for plants aged 5 years or less compared to those older than 20 years. This is consistent with many standard models of market selection (e.g., Jovanovic 1982; Hopenhayn 1992; Melitz 2003; Bartelsman, Haltiwanger, and Scarpetta 2013) where plants have a heterogeneous managerial capability when they are born, but there follows a rapid selection process where the weaker

[27] In Bloom et al. (2017), we showed that management predicts exits also at shorter horizons. The coefficients become monotonically more negative as the horizon becomes longer (by about 3 to 4 percentage points per year). Since establishment death is an absorbing state this is what we would expect.

[28] See Davis et al. (2014) for related evidence on this issue. They show that firms taken over by private equity downsize inefficient plants and expand efficient plants much more aggressively than other firms. As we show in Section III, there is substantial plant heterogeneity in management within the same firm, suggesting that changing management on the intensive margin may be hard to achieve easily.

[29] Growth between years s and t is calculated as $2 \times (L_t - L_s)/(L_t + L_s)$ following Davis and Haltiwanger (1992).

establishments exit the market (see Bloom, Sadun, and Van Reenen 2017, for an example of this type of model). When incumbent plants have matured to their steady state size, there is less of a relationship between growth and management practices (random management shocks will lead to some relationship).[30]

We also ran a series of other robustness tests on Tables 1 and 3, such as using standardized z-scores (rather than the 0–1 management scores), dropping individual questions that might be output-related and using ASM sampling weights, and found very similar results. We also looked at a nonparametric analysis of the management-size relationship (online Appendix Figure A2), finding a strongly positive relationship of management with both establishment size and firm size. This is also quantitatively large: average establishment size more than doubles going from about 50 employees for establishments with an average management score of approximately 0.52 (the twenty-fifth percentile) to about 120 employees for establishments with a management score of approximately 0.74 (the seventy-fifth percentile). Finally, we examined whether management could simply be proxying for other unobserved cultural or organizational features of the establishment (e.g., Gibbons and Henderson 2013). These are by nature hard to observe but in online Appendix Table A9 we look at decentralization (a measure of the distribution of power between the plant manager and corporate headquarters) and data-driven decision making. While these are informative in terms of productivity, our management indicator remains robust to including these as additional controls.

E. *Magnitudes of the Management and Productivity Relationship*

To get a better sense of the magnitudes of the relationship between management practices and productivity, we compare management practices to other factors that are commonly considered important drivers of productivity: R&D (Research and Development spending), Information and Communication Technologies (ICT), and human capital. We focus on these three because they are leading factors in driving productivity differences (e.g., discussed in detail in the survey on the determinants of productivity in Syverson 2011), and because we can measure them well using the same sample of firms used for the analysis of the management practices-productivity link. In particular, we ask how much of the productivity spread can be accounted for by the spread of management practices, R&D expenditure per worker, ICT investment per worker (spending on information and communication technology hardware and software), and human capital (measured as the share of employees with a college degree). We do this analysis at the firm level as establishment-level R&D is not the appropriate level for multi-plant firms.

Columns 1–4 of Table 4 report the results from firm-level regressions of log labor productivity (value added per worker) on those four factors individually. All of these factors are positively and significantly related to productivity. To obtain an aggregate firm-level labor productivity measure, the dependent variable is calculated as the weighted (by the plant shipment share of firm shipments) industry-demeaned

[30] Note that it is not obvious why TFP should be any more strongly related to management for young firms under this class of models. Indeed, in online Appendix Table A8 we do not find any systematic relationship in the TFP-management relationship by plant age.

TABLE 4—DRIVERS OF PRODUCTIVITY VARIATION

	Firm level log(labor productivity)				
	(1)	(2)	(3)	(4)	(5)
Management score	0.864				0.612
	(0.043)				(0.043)
R&D		0.133			0.095
		(0.010)			(0.010)
ICT/worker			0.062		0.047
			(0.006)		(0.006)
Skills (percent employees with college degree)				0.800	0.208
				(0.064)	(0.060)
Observations	~18,000	~18,000	~18,000	~18,000	~18,000
Share of 90–10 explained	0.216	0.216	0.120	0.159	0.441
Share of SD explained	0.193	0.219	0.134	0.142	0.282

Notes: OLS coefficients with standard errors in parentheses (clustered at the firm level). Dependent variable is firm level log(Value Added/Employment) built from industry de-meaned plant-level log(Value Added/Employment) weighted up by plant's shipments. Right-hand-side variables are *Management score*, *R&D* from BRDIS measured as log(1 + R&D intensity) where R&D intensity is the total domestic R&D expenditure divided by total domestic employment, *ICT investment per worker* (1,000 × spending on information and communication technology hardware and software per employee), *Skill* measured by the share of employees (managers and non-managers) with a college degree. All of these variables are also weighted up to the firm level using plant's total value of shipments. Missing values have been replaced by 0 for *R&D* and by means for the other variables. Industry demeaning is at NAICS6 level. All regressions are weighted by the number of establishments in the firm. *Share of 90–10 explained* is calculated by multiplying the coefficient on the key driver variable (e.g., management in column 1) by its 90–10 spread and dividing this by the 90–10 spread of productivity. *Share of SD explained* corresponds to the square root of the R^2 in the regression.

plant-level labor productivity.[31] This is then regressed on the firm-level value of the management score in column 1. The bottom row of column 1 shows that the 90-10 spread in management practices accounts for about 22 percent of the spread in labor productivity. In columns 2 to 4 we examine R&D, ICT, and skills and find these measures account for 21.6 percent, 12 percent, and 15.9 percent of the 90–10 productivity gap, respectively. Column 5 shows that the role of management practices remains large in the presence of the other factors, and that jointly these can account for about 44.1 percent of the 90–10 productivity spread in productivity. Similar conclusions come from other ways of accounting for productivity dispersion. For example, the contribution of each factor to the standard deviation of firm log(value added per worker) is 19.3 percent (management), 21.6 percent (R&D), 13.5 percent (ICT), and 14.2 percent (skills). The results in Table 4 highlight that our measure of management practices can account for a relatively large share of firm-level productivity.

There are several alternative approaches to looking at magnitudes. First, we used TFP[32] instead of labor productivity even though this is problematic as we are now summing across plants in industries with heterogeneous technologies when aggregating to the firm level. Nevertheless, the contribution of each factor to the 90–10 spread is similar to Table 4: 18.1 percent (management practices), 16.9 percent (R&D), 7.5 percent (ICT), 11.1 percent (skills), and 32.5 percent

[31] To obtain the firm-level measure of the right-hand-side variables, we weight the right-hand variables by their plant's share of total shipments (exactly as we do for the dependent variable). Results are robust to using the non-demeaned measure or other weighting schemes.

[32] We use a two-factor estimate of TFP in these calculations to be consistent with Table 4 which uses value added as the dependent variable.

(all 4).[33] Second, we can simply run the analogous production functions of Table 1, but at the firm level instead of plant level. Online Appendix Table A10 does this. Although the absolute level of the contribution of management practices (and the other factors) falls compared to Table 4, the relative contribution of management practices continues to remain as large as that of R&D and larger than that of ICT or skills.

One obvious caveat throughout this management practices and performance analysis is causality, which is hard to address with this dataset. In related work, Bloom et al. (2013) run a randomized control trial varying management practices for a sample of Indian manufacturing establishments with a mean employment size of 132 (similar to our MOPS sample average of 167). They find evidence of a large causal impact of management practices towards increasing productivity, profitability, and firm employment. Other well-identified estimates of the causal impact of management practices, such as the RCT evidence from Mexico discussed in Bruhn, Karlan, and Schoar (2018) and the management assistance natural experiment from the Marshall plan discussed in Giorcelli (2019), find similarly large impacts of management practices on firm productivity.

Given the evidence of the strong relationship between establishment performance and management, after briefly examining variation in management practices within firms we then turn to looking at two drivers of structured management practices where we believe we have credible causal identification.

III. Management Practices across Plants and Firms

One important question is to what extent do these variations in management practices across plants occur *within* rather than *between* firms? The results in Tables 1 and 3 suggest that there is enough within firm (across plant) variation even in the cross section to uncover a relationship between plant productivity and plant management. The voluminous case-study literature on management practices[34] often highlights the importance of variations both within and between organizations, but until now it has been challenging to measure these separately due to the lack of large samples with both firm and plant variation.

The benefit of the MOPS sample in addressing this question is twofold. First, the large sample means we have thousands of firms with multiple plants. Second, thanks to 500 double plant surveys we can control for measurement error, which would otherwise inflate the within-firm cross-plant variation. Armed with the earlier estimates that 45 percent of the variation in measured management was measurement error, we can now decompose the remaining variation in the management score into the part accounted for by the firm and the part accounted for by the plant. To do this, we keep the sample of 16,500 plants (out of 32,000 plants) that are in firms with 2

[33] About 50 percent of firm-level TFP appears to be measurement error according to Collard-Wexler (2013) and Bloom et al. (2018). Under the assumption that this measurement error is uncorrelated with the factors in Table 4 this implies these four factors can potentially account for about two-thirds of the true (non-measurement error) variation in TFP.

[34] For example, Brynjolfsson and Milgrom (2013) cite 11 case studies about variations in management practices and performance including Berg and Fast (1975); Barley (1986); Brynjolfsson, Renshaw, and van Alstyne (1997); and Autor, Levy, and Murnane (2002).

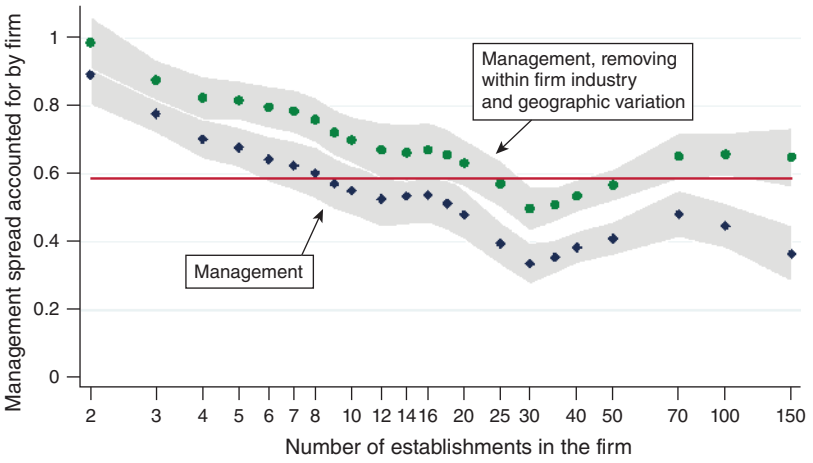

FIGURE 3. THE FIRM-LEVEL SHARE OF THE VARIATION IN MANAGEMENT SCORES (AFTER REMOVING MEASUREMENT ERROR)

Notes: Dots show the share of management score variation accounted for by the firm with different numbers of manufacturing establishments ranging from that number to the next value. So, for example, 50 plants refers to 50–74 plants. The share of variation is shown after removing the 45.4 percent accounted for by measurement error. The bootstrap sampled 95 percent confidence interval shown in gray shading. Sample of 16,500 establishments across the 3,100 firms with 2 or more plants in the 2010 MOPS survey. Industry variation is captured by six-digit NAICS dummies and geographic variation by MSA dummies (State is the MSA if MSA is missing). The horizontal line is the average share of the variation in score management across plants accounted for by firms, which is 58 percent.

or more plants in the MOPS survey in 2010. Although this sample only contains 44 percent of the overall number of observations in the sample, these are larger plants and account for 74 percent of output in the MOPS sample.

The first series in Figure 3 (diamonds) plots the share of the plant-level variation in the management score accounted for by the parent firm in firms with 2 or more plants after scaling by $(0.546 = 1 - 0.454)$ to account for measurement error. To understand this graph, first note that the top left point is for firms with exactly two plants. For this sample, firm fixed effects account for 90.4 percent of the adjusted R^2 in management variation across plants,[35] with the other 9.6 percent accounted for by variation across plants within the same firm. So, in smaller two-plant firm samples, most of the variation in management practices is due to differences across firms.

Moving rightward along the x-axis in Figure 3, we see that the share of management variation attributable to the parent firm declines as firm size rises.[36] For example, in firms with 50–74 plants, the parent firm accounts for about 40 percent of the observed management variation, and in firms with 150 or more plants, the parent firm accounts for about 35 percent of the variation. Hence, in samples of plants

[35] It is essential for this part of the analysis that the adjusted R^2 on the firm fixed effects is not mechanically decreasing in the number of establishments in the firm. To alleviate any such concern, we simulated management scores for establishments linked to firms with the same sample characteristics as our real sample (in terms of number of firms and number of establishments in a firm), but assuming *no firm fixed effects*. We then verified that indeed for this sample, the adjusted R^2 is zero and does not show any pattern over the number of establishments in a firm.

[36] The number of establishments on the x-axis is calculated using the LBD, counting all manufacturing establishments associated with the parent firm.

from larger firms, there is relatively more within-firm variation and relatively less cross-firm variation in management practices. The horizontal solid red line plotted shows the average share of variation in management scores across plants accounted for by the parent firm in our sample, which is 58 percent.

At least two important results arise from Figure 3. First, both plant-level and firm-level factors are important for explaining differences in management practices across plants, with the average share of management variation accounted for by firms being 58 percent (so 42 percent is across plants within the same firm). Second, the share of management practice variation accounted for by the parent firm is declining in the overall size of the firm, as measured by the number of establishments.

What explains the large fraction of within-firm variation in management practices? One likely explanation is that within a firm, different establishments operate in different environments: for example, different industries or locations. To evaluate this explanation, the second series in Figure 3 (green dots) repeats the analysis with one change: when we run the regressions of management on firm fixed effects (used to recover the adjusted R^2), we control for the part of the management score that is explained by within firm/across plant industry and MSA variation.[37] This essentially removes the within-firm share of variation in management that is explained by industry and geographical variation. There are two points to highlight from this exercise. First, by construction, the overall within-firm management variation is smaller, going down from 42 percent on average to 19 percent. Second, the relation between size and within-firm variation is flatter. Although we see a clear downward slope for firms with under ten plants, we cannot reject the null that the within-firm variation is similar for all firms with ten or more plants. This is consistent with larger firms (those with more than ten plants) operating across more industries and geographical regions, which accounts for their greater within firm spread in management practices.

We further explore these points in Table 5, reporting results from a regression of the within-firm standard deviation of the management score on firm level characteristics. Consistent with the first series in Figure 3 (blue diamonds), column 1 demonstrates that the standard deviation of management within a firm is increasing with the number of establishments in the firm, and that this relation is stronger for firms with 10 establishments or less (column 2). Column 3 shows that operating in more industries and over more locations are both correlated with a larger within-firm spread of management. Column 4 is consistent with the results in the second series in Figure 3 (green dots): controlling for the number of within-firm industries and locations, the relationship between management spread and size weakens and becomes insignificant for firms with more than ten manufacturing establishments. Column 5 shows that the within-firm spread of management is larger with more ownership changes.[38]

[37] Specifically, the R^2 regressions include now the linear projection of management from a regression of management on full sets of NAICS and MSA dummies (where for plants in areas without an MSA, the state is used), where the regression also includes firm fixed effects. The sample for this regression is identical to both series in Figure 3.

[38] We also looked at ownership changes over longer periods and found evidence suggesting it takes at least three years after a firm acquires a new plant to significantly change its management practices (consistent with earlier evidence in Bloom, Sadun, and Van Reenen 2012).

Table 5—Within-Firm (and Across-Plant) Variation in Management

Dependent variable:	Standard deviation of management spread within firm				
	(1)	(2)	(3)	(4)	(5)
Number of manufacturing establishments (in logs)	0.959				
	(0.091)				
Number of manufacturing est. × (10 establishments or smaller)		1.370		0.560	0.593
		(0.231)		(0.284)	(0.283)
Number of manufacturing est. × (larger than 10 establishments)		0.679		0.133	0.112
		(0.204)		(0.231)	(0.23)
Number of manufacturing industries (in logs)			0.378	0.29	0.269
			(0.147)	(0.171)	(0.171)
Number of manufacturing states (in logs)			1.040	0.883	0.872
			(0.140)	(0.186)	(0.186)
Share of MOPS est. with ownership change in the prior year					1.650
					(0.808)
Number of firms	~3,100	~3,100	~3,100	~3,100	~3,100

Notes: A firm-level regression with the standard deviation of management scores across establishments within the firm as the dependent variable. The regression sample is all firms with two or more establishment responses in the MOPS 2010 survey. The total number of establishments, the number of establishments within manufacturing, the number of different industries, and the different number of states these establishments span are all calculated from the Longitudinal Business Database (LBD). Change of ownership is defined as share of MOPS establishments with a different FIRMID as compared to a base year's LBD (e.g., LBD 2009 for 09–10). In all columns, we control for a fifth-degree polynomial of average management score at the firm. Robust standard errors are reported in parentheses. For scaling purposes, all coefficients and standard errors have been multiplied by 100.

The importance of geographical location stands out in Table 5, being significant across all columns. This motivates us to consider geographical factors that may help explain the wide variation of management practices across plants.

IV. Drivers of Management Practices

The previous literature on management has pointed to a wide variety of potential factors driving management practices. We focus on two, business environment and learning spillovers, for which we have credible identification strategies and significant spatial variation. Online Appendix B describes a simple model to help interpret the coefficients of the effect of these drivers on management (and other outcomes like measured TFP).

A. *Business Environment*

The business environment in which plants operate is often thought to be a major factor for understanding the variation in management across plants. As a measure of the business environment, we use right-to-work (RTW) laws, which are state-level laws prohibiting agreements between employers and labor unions that require employees' membership, payment of union dues, or fees as a condition of employment, either before or after hiring. They now cover 28 states, and have been growing in coverage. We use two identification strategies to examine the causal impact of RTW on management. First, we use a DID approach exploiting the introduction of RTW in two states in 2012. Second, in the spirit of Holmes (1998) we use a spatial RD design around state boundaries.

Difference-in-Differences (*DID*).—In 2012, two US states (Michigan and Indiana) introduced RTW laws. Since we have waves of the management survey in 2010 and 2015 we can run a DID analysis of management changes between 2010 and 2015 comparing these states to their neighbors. The treatment states are compared to their contiguous neighbors: Ohio, Illinois, and Kentucky.[39] We do not use the neighboring state of Wisconsin as it introduced right-to-work laws right at the end of our panel (in 2015). Three other states, West Virginia (2016), Kentucky (2017), and Missouri (2017), introduced right-to-work laws after our sample period.[40] This enables us to run a placebo test on these three states to examine whether other events triggering a successful RTW vote could be influencing management rather the RTW laws themselves.

Our empirical analysis relies on a standard DID specification,

$$M_{it}^{m} = \theta_1\left(RTW_s \times POST_t\right) + \theta_2 X_{i,t} + \omega_s + \tau_t + \epsilon_{it}, \tag{3}$$

where M_{it}^{m} indicates the management practice score of plant i in year t, the superscript m indicates whether we are considering subsets of the management score such as incentives practices, RTW_s is a dummy for the two RTW states, $POST_t$ is a dummy for the years after the introduction of RTW in 2012 (i.e., a dummy for 2015), $X_{i,t}$ are other observable controls (NAICS6 dummies and a recall dummy), ω_s are state dummies, τ_t are time dummies, and ϵ_{it} is an error term.

Table 6 contains the results for our baseline specification in panel A. Column 1 reports a positive but insignificant coefficient on the treatment variable. RTW is likely to primarily affect "incentives practices" over human resources such as tying pay, firing, and promotion to employees' ability and performance. Unions frequently oppose these practices which they believe give too much discretion to employers, so if unions are weakened by RTW then these incentives practices will likely become more prevalent. Consequently, column 2 looks specifically at these incentive practices as an outcome (questions 9 through 16 in the MOPS questionnaire). The coefficient is twice as large as column 1 and significant at the 5 percent level in this specification. In column 3 we examine non-incentive management practices (the other 8 MOPS questions on monitoring and targets, which are much less directly related to RTW laws), and find a precisely estimated 0 coefficient. The test for equality of these coefficients rejects equality at the 10 percent level (p-value $= 0.053$).

In column 4 of Table 6, we examine unionization directly as the most likely mechanism through which the RTW effect might operate. In the MOPS survey, union density is in bins so we create a dummy indicating where 80 percent or more of workers are union members. RTW has a negative and significant effect on the prevalence of these highly unionized plants. This is as expected since the introduction of

[39] The large sample size (35,000 plants) for each MOPS wave, plus the high density of manufacturing plants in these states, means that pooling over 2005, 2010, and 2015 we have about 17,000 observations for this DID regression analysis.

[40] In Missouri, RTW legislation was signed into by Governor Eric Greitens in February 2017, but implementation was postponed as there was a successful petition to hold a public referendum over the law. On August 7, 2018 voters on the referendum opposed the law.

Table 6—Difference-in-Differences Estimates for the Effect of Right-to-Work

	Overall management (1)	Incentive management (2)	Non-incentive management (3)	High union (density > 80%) (4)	log(emp) (5)	3 Factor log(TFP) (6)
Panel A. DID estimates for the effect of right-to-work						
Post × Treat	0.006	0.013	−0.000	−0.019	0.117	0.016
	(0.005)	(0.006)	(0.005)	(0.007)	(0.026)	(0.013)
Panel B. Placebo in states in years before right-to-work introduction						
Post × Treat	0.007	0.014	0.002	−0.020	0.132	0.018
	(0.005)	(0.006)	(0.005)	(0.007)	(0.027)	(0.013)
Pre × Treat	−0.004	−0.002	−0.007	0.002	−0.057	−0.006
	(0.006)	(0.007)	(0.007)	(0.010)	(0.033)	(0017)
Panel C. DID estimates controlling for NAICS trends and state trends						
Post × Treat	0.016	0.021	0.003	-0.015	0.200	0.029
	(0.008)	(0.011)	(0.006)	(0007)	(0.049)	(0.024)
Observations	∼17,000	∼17,000	∼17,000	∼17,000	∼17,000	∼17,000
Panel D. Placebo using West Virginia, Kentucky, and Missouri						
Post × Treat	0.001	0.004	−0.001	−0.007	0.007	−0.013
	(0.005)	(0.007)	(0.006)	(0.008)	(0.028)	(0.016)
Observations	∼27,000	∼27,000	∼27,000	∼27,000	∼27,000	∼27,000

Notes: OLS coefficients with standard errors in parentheses (clustered at the establishment level). *Post* is a dummy for 2015, and in panels A through C *Treat* is an indicator for Michigan and Indiana (states which introduced RTW in 2012), so *Post* × *Treat* identifies the effect of the policy change. *Pre* is a dummy for 2010 so *Pre* × *Treat* in panel B is a test for pre-policy introduction trends. In panel D, *Treat* is a dummy for West Virginia, Kentucky, and Missouri (three states which passed RTW laws after 2015) so this is an alternative placebo. The dependent variable is the overall 16 question management score in column 1. In columns 2 and 3 the score is calculated as the unweighted average of the incentives related practices (MOPS questions 9–16) and non-incentives related practices (MOPS questions 1–8) respectively. The dependent variable in column 4 is calculated using the categories in MOPS question 36 (2010 numbering). The dependent variable in column 5 is log of employment at the establishment, and in column 6 the log of total factor productivity, calculated using a factor share of three factors (capital, labor, and material). The sample in panels A through C is all MOPS observations with at least ten non-missing responses to management questions and a successful match to ASM, which were also included in ASM tabulations, have positive value added, positive employment, and positive imputed capital in the ASM from treated and neighboring states for the years 2005, 2010, and 2015. Recalls are used for respondents with at least seven years of tenure at the establishment. The sample in Panel D is defined similarly to the Panel A sample, but for the placebo states and their neighboring states. All regressions include year and state fixed effects, NAICS dummies, and a recall dummy. In panel C, we also include state trends and NAICS trends.

RTW eliminates the need for all employees to be a union member.[41] Columns 5 and 6 look at two performance related outcomes. Column 5 shows that RTW has a positive and significant effect on size of establishments as measured by employment, and column 6 shows a small, but insignificant positive effect of about 1.6 percent on TFP. The absence of a RTW effect on measured TFP may seem surprising given the positive effects on management, but one potential explanation is the greater entry of plants in RTW states increases demand for scarce inputs and so drives up relative

[41] We also checked whether the effect was larger for establishments in industries characterized by higher union density (as measured pre-law changes in 2010). We have found no robustly significant difference in the response to RTW laws across industries in this dimension. For example, in the specification of column 2 in panel C, including an interaction of Post × Treat with a dummy variable indicating whether the establishment's industry had union density in the top quartile generated a coefficient (standard error) of 0.007 (0.014).

input prices (such as land and local materials prices).[42] These "congestion effects" will tend to bias measured TFP downward and in principle, a driver could even appear to have a negative effect. This is a more general concern with TFP and we discuss this issue in more detail below in relation to MDPs (see online Appendix B for a formal discussion).

An obvious concern with the DID strategy is that there may be pre-policy trends, so that incentives management, employment, and productivity might have increased even in the absence of the RTW policy change. To assess this we include Pre × Treat as an additional control where Pre is a dummy for 2010. The coefficient on Pre × Treat reflects a pseudo-experiment as if a RTW vote was passed between 2005 and 2010 in the states who actually passed the laws in 2012. In panel B of Table 6 we find that there is no significant effect, which is consistent with the hypothesis of no pre-policy trends contaminating the treatment effects. Panel C presents an even more rigorous test which takes the specification from panel A and interacts the full set of NAICS6 dummies and state dummies with linear time trends. The results in panel C are broadly similar to the previous ones with a larger and (weakly) significant coefficient on incentives management in column 2, but not for non-incentives management in column 3.

Finally, as noted above, West Virginia, Kentucky, and Missouri introduced right-to-work laws, but after 2015, the last year of our panel. This enables us to run a placebo test on the voting for (but not the introduction of) RTW legislation. This is to further address the concern that there may be unobservables correlated with the holding of a RTW vote which could confound our treatment effects. We replicate our baseline specification (panel A) and again use contiguous states as controls.[43] Panel D of Table 6 contains these placebo results and shows that there is no significant treatment effect on any of the outcomes.[44]

Regression Discontinuity (RD) Design.—At the time of the 2010 MOPS survey, 22 states had RTW laws in place, mostly in the South, West, and Midwest. In Table 7, we compare plants in counties that are within 100 miles of state borders that divide states with different RTW rules. We estimate the following equation:

$$M_i^m = \theta_1 D_i + \theta_2 X_i + \varphi_B(DISTANCE_i) + B_{s,s'} + e_i, \tag{4}$$

where D_i is a dummy variable for whether the firm is located in a state with a RTW law, X_i are the other observable controls (such as NAICS6 and recall dummies), $\varphi_B(DISTANCE_i)$ is a polynomial function of a plant's distance to a state border (which we allow to take a different shape on either side of the border as indicated by the B subscript), and $B_{s,s'}$ are 74 border dummies, specific to every pair of states with a different RTW regime. Since we have multiple years we define variables specific

[42] If this entry also increased local product market competition this would also generate a downward bias to our revenue based measure TFP measure as mark-ups would shrink.

[43] These states are Virginia, Maryland, Pennsylvania, Tennessee, Illinois, Ohio, Arkansas, Oklahoma, Kansas, Nevada, and Iowa.

[44] We replicate panel A specification as this is the more conservative approach to the placebo. However, the results are very similar when repeating the placebo analysis using the specification from panel C. Specifically, no treatment coefficient is significant by any standard, and magnitudes are broadly similar. For example, the coefficient (standard error) on the treatment dummy in column 2 of panel D becomes 0.006 (0.011).

TABLE 7—RD DESIGN ESTIMATES FOR THE EFFECT OF RTW

	Management score (1)	Incentive management (2)	Non-incentive management (3)	High union (density > 80%) (4)	log(emp) (5)	3 factor log(TFP) (6)
Panel A. RD design estimates						
RTW side of the border	0.008	0.017	0.002	−0.042	0.110	−0.012
	(0.006)	(0.006)	(0.007)	(0.006)	(0.054)	(0.012)
Observations	~39,000	~39,000	~39,000	~39,000	~39,000	~39,000
Panel B. RD design estimates controlling for 6-digit NAICS						
RTW side of the border	0.007	0.014	0.002	−0.038	0.034	−0.008
	(0.004)	(0.005)	(0.005)	(0.007)	(0.034)	(0.012)
Observations	~39,000	~39,000	~39,000	~39,000	~39,000	~39,000
Panel C. RD design estimates for non-tradables (25 percent lowest HHI in sample)						
RTW side of the border	0.014	0.020	0.012	−0.031	0.080	−0.053
	(0.008)	(0.009)	(0.010)	(0.009)	(0.042)	(0.024)
Observations	~9,200	~9,200	~9,200	~9,200	~9,200	~9,200
Panel D. RD design estimates allowing quadratic distance functions						
RTW side of the border	0.007	0.013	0.004	−0.051	0.037	−0.017
	(0.005)	(0.005)	(0.006)	(0.012)	(0.038)	(0.012)
Observations	~39,000	~39,000	~39,000	~39,000	~39,000	~39,000
Panel E. RD design estimates with Epanechnikov weights for distance from border						
RTW side of the border	0.008	0.016	0.003	−0.040	0.031	−0.011
	(0.004)	(0.005)	(0.006)	(0.007)	(0.034)	(0.012)
Observations	~39,000	~39,000	~39,000	~39,000	~39,000	~39,000

Notes: OLS coefficients with standard errors in parentheses (clustered at the establishment level). The dependent variable is the management score in column 1. In columns 2 and 3, the score is calculated as the unweighted average of the incentives related practices (MOPS questions 9–16) and non-incentives related practices (MOPS questions 1–8) respectively. The dependent variable in column 4 is calculated using the categories in MOPS question 36 (2010 numbering). The dependent variable in column 5 is log of employment at the establishment, and in column 6 the log of total factor productivity, calculated using a factor share of three factors (capital, labor, and material). The sample in panels A, B, D, and E is all MOPS observations with at least ten non-missing responses to management questions and a successful match to ASM, which were also included in ASM tabulations, have positive value added, positive employment, and positive imputed capital in the ASM from RTW states or states with a RTW border. Recalls are used for respondents with at least seven years of tenure at the establishment. Sample in Panel C is defined similarly to Panel A, but restricted to the 25 percent of the sample from most tradable industries, where non-tradables are defined as industries with low regional concentration level calculated following Ellison and Glaeser (1997) using data from the 2007 census. All regressions include a recall dummy and year and border fixed effects. Panels B through E also include NAICS dummies. Panels A, B, C, and E allow for different linear trends on each side of the border, while panel D allows for different quadratic trends. Panel E applies Epanechnikov kernel for weighting observations according to distance from the border.

to the year and add time dummies (and a recall dummy) to the regression.[45] We have 39 states who are either RTW states or their neighbors and cluster the standard errors at the state level.

Unlike a classic RD design, the location of the plant across the discontinuity (border) *can* be manipulated by agents. So the treatment effect we identify is a combination of any effects on existing plants plus the selection of plants with more

[45] Results are almost identical when we generalize the model to include border pair interacted by time dummies.

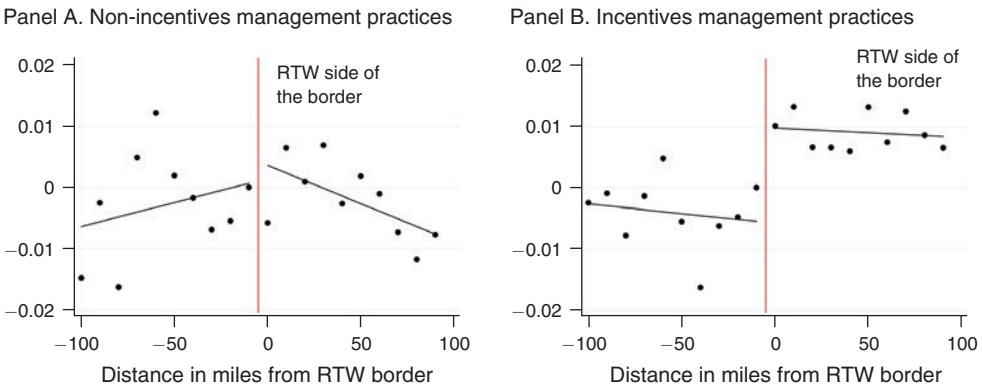

Figure 4. Right-to-Work Regression Discontinuity

Notes: The management measure in panel A is calculated as the unweighted average of the non-incentives related practices (MOPS questions 1–8), and in panel B as the incentives related practices (MOPS questions 9–16). The sample is all MOPS observations with at least ten non-missing responses to management questions and a successful match to ASM, which were also included in ASM tabulations, have positive value added, positive employment, and positive imputed capital in the ASM from RTW states or states with a RTW border. Recalls are used for respondents with at least seven years of tenure at the establishment. The figures show ten mile bin dots.

structured incentive management into RTW states. Furthermore, recall that θ_1 will reflect the effect of the entire bundle of state specific policies on either side of the border, not just RTW laws (the DID analysis is more specific in this respect). The identification here is essentially cross-sectional and relies on comparing at a point in time establishments on two sides of the border. The key identification assumption is that as we shrink distance to zero, the non-state policy related factors (e.g., economic and geographical) become identical on either side of the border.

Figure 4 shows the RD design visually. Panel A looks at average non-incentive management practices for various distances away from the border. There is no apparent discontinuity in the adoption of non-incentive practices around RTW border in the data. In panel B we look specifically at incentive management practices and observe a clear discontinuity in incentives management at the state boundary. This is consistent with a causal effect. Interestingly, the incentive management scores look broadly stable as we move away from the border. If there were very local selection effects so that the impact of state policies was to switch only a few highly structured management plants across the border, we might expect to see some bunching (a sudden increase in average management scores as we approach the border), which we do not observe.[46]

Table 7 reports similar outcomes to the ones reported in the DID estimates of Table 6 but for estimates from the RD design, allowing for different trends in distance on two sides of the border. In columns 1–3 of Table 7, the regression sample includes all plants in bordering pairs within 100 miles of a state-border between two states with different RTW laws. In panel A, we see that the plants on the RTW side of the border have significantly higher incentive management scores, but there are

[46] Note that we do see more plants on the RTW side of the border, i.e., there is a bigger mass on the RTW side of the border, though this is not driven by bunching at the border.

no significant effects on other types of management practices. The magnitude of the effect is similar to the magnitude from the DID analysis. For example, the treatment effect in column 2 is 0.017, slightly larger compared to the same estimate in column 2 of Table 6. Given that these are from different identification strategies, this similarity is reassuring. RTW reduces union density according to column 4. We also find significant positive effects on employment in column 5, but again no significant effects on TFP.

Panel B of Table 7 includes NAICS6 dummies and shows robust effects of RTW (NAICS dummies are included in panels C to E as well). Specifically, the difference between incentive and non-incentive management is still large, with the test for equality of the two coefficients rejected at the 1 percent level (p-value $= 0.007$). Note that the magnitudes of management coefficients are even closer to the ones reported in panel A of Table 6 (which also controls for NAICS). As noted above, the RD coefficient reflects both pure treatment effects on incumbents and the fact that plants with more incentive management practices will likely sort onto the RTW side of the border. From a state policy perspective, these sorting effects are of interest, but if all of the effect is selection through cross border switching then this may mean the equilibrium impact of the policy is zero. Furthermore, even from a purely local perspective, the effect is over-estimated because some of the impact may be coming from lower structured management scores in the non-RTW states due to the movement of plants with more structured practices to the RTW side of the border. So in panel C we look at plants in the least tradable quartile of industries (industries like cement, wood pallet construction, or bakeries, defined in terms of being in the bottom quartile of geographic concentration) that are the least likely to select on location because of high transport costs.[47] Again, we find RTW states have significantly higher structured management scores within this sample of relatively non-tradable products for which selecting production location based on "business-friendly" conditions is harder. This, coupled with the similarity of the effect for plants further from the border and the DID effects on incumbents, suggests that the management effects of RTW are not primarily due to cross-border switching. The last two panels of Table 7 report technical robustness tests for the RD estimates. In panel D we replace the linear trends in distance from border with quadratic trends, and in panel E we weight observations using the Epanechnikov kernel applied to (absolute value of) distance from border. As these panels show, the results are not sensitive to the econometric details of the RD design specification.

B. *Learning Spillovers: Million Dollar Plants*

Do structured management practices "spill over" from one establishment to another? We would expect this to happen if there is learning behavior, making management qualitatively different from other factor inputs. To get closer to a causal effect, we study how management practices in particular counties in the United States change when a new, large, and typically multinational establishment (likely

[47] Our industry geographic concentration indexes are calculated following Ellison and Glaeser (1997) using the 2007 Census of Manufactures.

to have higher management scores) is opened in the county.[48] A key challenge, of course, is that such counties are not selected at random. It is in fact very likely that counties that "won" such large multinational establishments are very different than a typical county in the United States. To overcome this issue, we compare counties that "won" the establishment with the "runner-up" counties that competed for the new establishment. This approach is inspired by Greenstone, Hornbeck, and Moretti (2010), who study the effect of agglomeration spillovers by looking at productivity of winners and runner-up counties for Million Dollar Plants (MDPs). We used *Site Selection* magazine to find Million Dollar Plants as described by Greenstone, Hornbeck, and Moretti (2010), extending the list by web searching for MDP counties and runner-ups (see online Appendix A for more details about data construction), with our full MDP data available online.[49]

Following our data structure of a five-year panel, we estimate the following equation:

$$\Delta M_{icst} = \theta_1 \Delta MDP_{ct} + \theta_2 X_{icst} + P_{c,c'} + f_s + e_{it}, \tag{5}$$

where ΔM_{icst} is change in the management score for establishment i in county c, state s between year $t-5$ and year t, ΔMDP_{ct} is a dummy that equals 1 if the county had an MDP opening between years $t-5$ and t, X are other observable controls, and $P_{c,c'}$ are 45 dummies, specific to every pair of winning and losing counties, and f_s are state fixed effects. The MDP opening year for the regression is set to be the first year the MDP shows up in the LBD in cases where the establishment is new (rather than an expansion) and was successfully matched to the LBD. Otherwise we use the announcement year + 1. We use 2005–2008 MDP openings for the 2005–2010 changes, and 2010–2013 openings for the 2010–2015 changes to allow one or two years before any meaningful managerial effects occur.

Before looking at the results, we check that the observable characteristics for winners and runner-up counties are balanced (see online Appendix Table A11). We look at all MDPs pooled in column 1 and then separately for establishments with high and low manager flow between the establishment and the MDP industry codes in the next two columns. Of the 33 coefficients, only 5 are statistically significant. Importantly, there are no significant differences in $t-10$ to $t-5$ *trends* in employment, productivity, value added, and county characteristics between winners and runner-ups.

Table 8 contains the spillover results with the baseline results in panel A. Column 1 suggests a positive and significant effect of MDPs on management. In contrast to our RTW results, there are significant effects on both incentives and non-incentives management.[50] This is unsurprising, as there are no ex ante reasons to believe effects of MDPs should be larger on incentives management. Column 2 shows positive but statistically insignificant effects on TFP. As with the RTW case, this may be because

[48] Note that we *do not* choose these plant openings using Census data, but using public data only (see more details in the online Appendix A). In fact, to ensure the confidentiality of plants in our sample, we do not report whether these plants even appear in our data.

[49] See https://people.stanford.edu/nbloom/research. We are grateful to Hyunseob Kim for sharing an updated list of million dollar plants and discussing search strategies from his work, Kim (2015).

[50] The coefficients are of similar size at 0.019 (0.007) for non-incentives and 0.020 (0.011) for incentives management.

Table 8—Management Knowledge Spillovers

	Change in management (1)	Change in log(TFP) (2)	Employment growth (3)
Panel A. All industries pooled			
MDP opens	0.018	0.024	0.014
	(0.007)	(0.017)	(0.005)
Panel B. Split high/low manager flow			
MDP opens × high	0.031	0.069	0.017
	(0.008)	(0.019)	(0.006)
MDP opens × low	−0.005	−0.050	0.009
	(0.011)	(0.034)	(0.01)
p-value for equality	0.007	0.004	0.495
Panel C. Split high/low trade (*demand spillovers*)			
MDP opens × high	0.012	0.041	0.029
	(0.011)	(0.033)	(0.010)
MDP opens × low	0.023	0.004	0.001
	(0.009)	(0.032)	(0.008)
p-value for equality	0.439	0.509	0.037
Panel D. Manufacturing MDPs split out			
MDP opens × manufacturing	0.016	0.040	0.011
	(0.007)	(0.016)	(0.006)
MDP opens × non-manufacturing	0.022	−0.016	0.022
	(0.015)	(0.038)	(0.008)
p-value for equality	0.734	0.175	0.297
Observations	~2,500	~2,500	~2,500

Notes: OLS coefficients with standard errors in parentheses (clustered at the county level). The sample is all MOPS observations with at least ten non-missing responses to management questions and a successful match to ASM, which were also included in ASM tabulations, have positive value added, positive employment, and positive imputed capital in the ASM in counties that were considered by Million Dollar Plants (MDPs) as part of the site selection process. The dependent variable is the change from $t-5$ to t. For column 1: *Change in management* score winsorized at top and bottom 1 percent, column 2 *Change in log*(*TFP*) truncated at the top and bottom 1 percent, calculated using factor share for three factors (capital, labor, and material), column 3 *Employment growth* defined as $2 \times (L_t - L_{t-5})/(L_t + L_{t-5})$. The key right-hand-side variable is a dummy indicating whether the plant was in the county finally selected for the plant location or not. All regressions have pair, states, industry, and recall fixed effects. Panel B interacts the treatment with high and low manager flow between the establishment and the MDP industries, and panel C splits using trade flows. High and low defined using medians, and the regression controls for the non-interacted *High* dummy. Panel D interacts the treatment with dummy indicating whether the MDP is in manufacturing or not. Each panel includes the *p*-value for equal coefficients over the split. All regressions are weighted by the MDP announcement employment size.

of plant entry driving up land and input prices in MDP counties, downward biasing measured TFP.[51] Column 3 uses employment growth as a dependent variable and shows positive and significant effects consistent with column 1.

Some plants are more likely to benefit from MDPs than others. In particular, if the MDP effect is really due to learning spillovers we would expect the benefits to be particularly strong if there are likely to be larger flows of managers between the MDP and local firms. To examine this, we pooled the Current Population Surveys (CPS) from 2003 to 2015 and examined the flows of managers between different

[51] Indeed, Greenstone, Hornbeck, and Moretti (2010) reported the impact of MDPs on land prices and wages, so that not only land but any local land or labor intensive inputs would see higher prices.

three-digit NAICS industries. For every MDP we can observe its industry code and whether a plant in the treatment (or control) county is in an industry that is more likely to benefit from a managerial flow (which are typically in similar industries). We then split plants into above versus below median management flows based on the plant and the MDP industry codes using the bilateral matrix for employees in managerial occupations from the CPS.[52] Panel B of Table 8 shows that the MDP effect is only statistically significant for plants in those industries that are more likely to receive a larger managerial flow from the MDP industry.[53] Consistent with this, we find that these "more exposed" plants also benefit from significantly higher TFP and jobs growth. Note that the spillover may occur in more subtle ways than simply the movement of managers from the MDP to local firm. Using national inter-industry managerial labor flows as a "distance metric" may also reflect that there will be greater interactions between MDP managers and those of local firms in professional and social situations.

We also estimated event study versions of MDP entry as their entry dates vary by year.[54] This is challenging because we only have (at most) three observations of management at five-year intervals, so there are large standard errors. Panel A of Figure 5 shows this for the simple MDP effect (the generalization of Table 8, panel A) which peaks significantly 3–4 years after entry. The drop in impact after the third year could reflect some gradual depreciation of the MDP impact as there is less to be learned by incumbent plants over time and the information about management practices may also start to diffuse to the plants in control counties. It could also reflect that we are mixing establishments who differentially benefit from the MDPs. Indeed, panel B of Figure 5 breaks down the MDP effect by predicted managerial labor flows (as in Table 8, panel B). There is no sign of an MDP effect in the years before and immediately on entry, but for establishments with high predicted managerial flows with these MDPs we see a significant spillovers after a year, while for plants with low predicted managerial flows we do not see significant spillovers.

There are many other ways to build up a distance metric between the MDP and the incumbent plants. Panel C of Table 8 uses the goods input-output matrix. We do not find much evidence of larger management to TFP spillovers associated with higher trade links, but we do find some larger employment effects. This is consistent with incumbent plants benefiting from a demand effect if an MDP is in a buyer-seller relationship, but not learning spillovers. Panel D looks at the product market dimension dividing MDPs into manufacturing versus non-manufacturing MDPs. Consistent with the idea that our manufacturing plants are more likely to benefit from manufacturing MDPs, only manufacturing MDPs are statistically significant. However, the coefficients on the two types of MDPs are not significantly different from each other.

[52] We use employees in occupation classification "Executive, Administrative, and Managerial Occupations," corresponding to occupation codes 003 to 037 in the IPUMS harmonized occ1990 variable.

[53] We also find a similar pattern using the bilateral flow matrix for all employees, but the results are weaker than just using managerial flows. Whereas the *p*-test of the significance between the two types of industries for managerial flows is significant at the 5 or 10 percent levels as shown in Table 8, a similar test for all employee flows has *p*-values of 0.23.

[54] We cannot implement event studies on right-to-work laws in Table 6 because both our treatment states introduced them in the same year (2012).

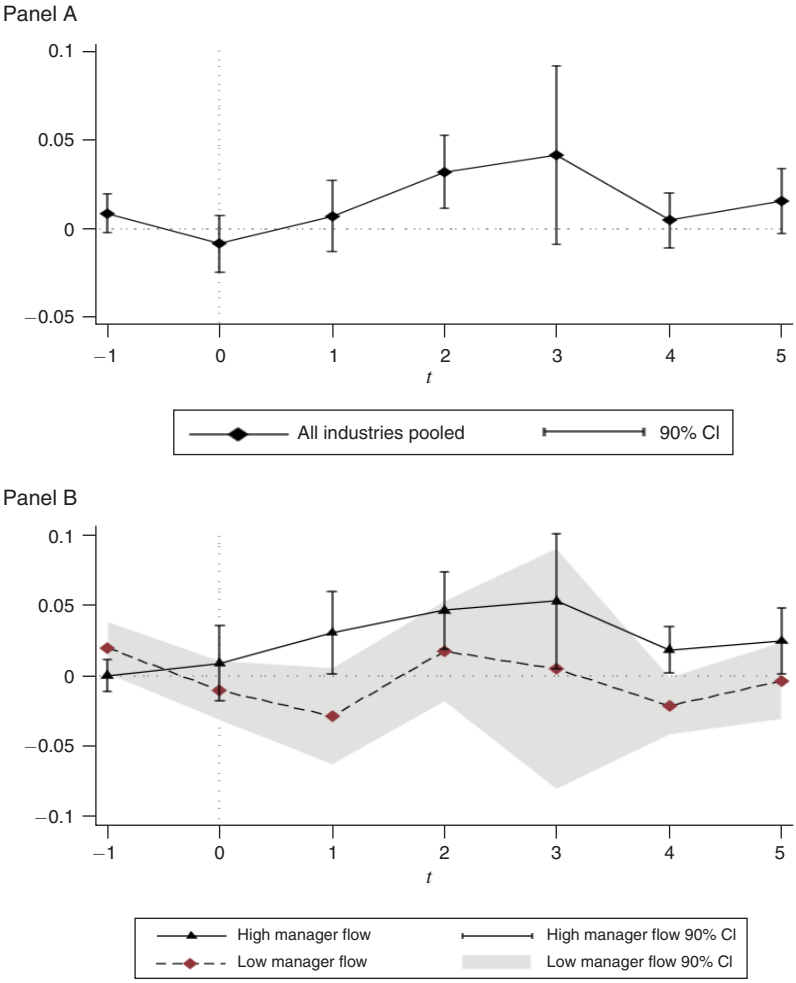

FIGURE 5. EVENT STUDIES OF IMPACT OF MILLION DOLLAR PLANTS ON INCUMBENT PLANTS

Notes: These are event studies estimated in a window of 1 year before the MDP arrives ($t = -1$) through to 5 years afterward ($t = 5$). Panel A is the dynamic version of the results in column 1 of Panel A in Table 8 with the same controls variables (dummy for each pair of MDP winner and loser, recall dummy, and NAICS and state dummies). Panel B allows MDP effect to differ by whether incumbent plant in industry where there is a high frequency of manager flows between the MDP's industry and the plant's industry (above median is *High* and below median is *Low*). Sample is all MOPS observations 10 or more nonmissing responses to management questions (recalls only considered if respondent had at least seven years of tenure). We also require (i) successful match to ASM; (ii) positive values of value added, employment, materials, and capital; (iii) all observations appear in at least two years (out of 2005, 2010, and 2015) in a county which either had an MDP established between 2005 and 2016 (winner), or competed for an MDP and lost (loser).

We conclude that MDPs do appear to have significant effects on management, but only if plants are closely connected as revealed through managerial labor markets (rather than just being in an input-output or product market relationship). These improvements in management also feed through into jobs and productivity gains.

C. *Discussion*

Through the lens of the simple model in online Appendix B there are at least two mechanisms through which the reduced-form evidence of our drivers could influence management practices. First, by reducing the effective "price" of adopting structured management practices RTW and MDP could increase management scores. This could then improve productivity as suggested by Table 1. However, an alternative story would be that RTW and MDP increased productivity through some non-management mechanism (e.g., the adoption of new technologies) and that this increase in productivity caused the firm to grow and therefore increase all factor inputs including managerial capital.

We cannot directly rule out this second mechanism with our data, but several pieces of evidence suggest that it is not the whole story. First, the RTW effect is not on all managerial practices, but specifically over those related to incentives, which is exactly where we would expect this specific regulation to have its largest effect. One might believe that incentives are easier to adjust than other types of management (although one could make the opposite case that pay and promotions are actually very sensitive organizational issues and are difficult practices to change). However, we can directly look at this by disaggregating the management score in the MDP analysis. As discussed above, here we find that if anything the MDP effects look stronger on the non-incentive aspects of management, such as lean manufacturing and monitoring. This is plausible as these aspects may be the harder ones to understand and implement in the absence of demonstration by another firm. Second, we can condition on employment growth to absorb the overall effects on size. These regressions must be interpreted with caution as we have an endogenous variable on the right-hand side, but it is striking that the coefficient on our treatment variable does not fall by much in these "conditional management-capital demand" equations.[55] Thirdly, it is worth noting that the effects of these drivers is generally stronger on management than it is on TFP, which is the opposite of what we would expect if they were affecting management via TFP.

D. *Other Drivers*

We have focused on two important drivers of management: business environment and learning spillovers. But there are many other potential factors. In Bloom et al. (2017), the NBER working paper version of this paper, we also analyze exogenous variation in two additional factors: education and competition. We find robust evidence that higher levels of human capital and competition are both positively associated with higher levels of the management scores.

[55] For MDP the coefficient changes from 0.018 in column 1 of Table 8, panel A to 0.017 with a standard error of 0.006; for the RD Design RTW in column 2 of Table 7, panel A it falls from 0.017 to 0.012 with a standard error of 0.005; and in the DID RTW of column 2 of Table 6, panel A it falls from 0.013 to 0.009 with a standard error of 0.006.

V. Conclusions and Future Research

This paper analyzes a recent Census Bureau survey of structured management practices in 2010 and 2015 for about 35,000 plants in each wave across the United States. Analyzing these data reveals three major findings. First, there is a large variation in management practices across plants, with about 40 percent of this variation being across plants *within* the same firm. This within-firm across-plant variation in management cannot easily be explained by many classes of theories that focus on characteristics of the CEO, corporate governance, or ownership (e.g., by family firms or multinationals) because these would tend to affect management across the firm as a whole.

Second, we find that these management practices are tightly linked to several measures of performance, and they account for about one-fifth of the cross-firm productivity spread, a fraction that is as large as or larger than technological factors such as R&D or IT. Furthermore, management practices are very predictive of firm survival rates, in fact, more so than TFP.

Third, we find causal evidence that two drivers are very important in changing management practices. The business environment (as measured by right-to-work laws) increases the adoption of structured incentives management practices. Learning spillovers as measured by the arrival of large new entrants in the county (Million Dollar Plants) increases the management scores of incumbents.

Although both of these drivers are qualitatively important across geographical regions, they cannot explain the large variation of management practices within the same region, much of which is within the same firm. This is not obviously due to firm-wide factors such as CEO identity or corporate governance. It is suggestive of the importance of frictions to within-firm changes in management and organization as discussed by Gibbons and Henderson (2013) and Milgrom and Roberts (1990) among others. While this paper has provided some answers concerning drivers of differences in management practices, there is still ample room for new theory, data, and designs to help understand one of the oldest questions in economics and business: why is there such large heterogeneity in management practices?

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