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## Embracing at arm's length: Ronald Coase's uneasy relationship with the Chicago School

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EMBRACING AT ARM'S LENGTH: RONALD COASE'S UNEASY  
RELATIONSHIP WITH THE CHICAGO SCHOOL

BY STEVEN G. MEDEMA

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**Embracing at Arm's Length:  
Ronald Coase's Uneasy Relationship with the Chicago School**

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\* Department of Economics, Duke University. Email: [steven.medema@duke.edu](mailto:steven.medema@duke.edu). I would like to thank Craig Freedman for permission to quote from his October 1997 interview with Ronald Coase, Ning Wang for insights into aspects of Coase's thinking on this score, as well as Roger Backhouse, James Forder, Daniel Hammond, Doug Irwin, two anonymous referees, and participants at the 2018 UK History of Economic Thought Conference for very instructive comments on earlier drafts of this paper. A special word of thanks goes to the staff of the Special Collections Research Center at the University of Chicago's Regenstein Library for their unfailingly generous assistance during my visits. The usual caveat applies.

## **Embracing at Arm's Length:**

### **Ronald Coase's Uneasy Relationship with the Chicago School**

#### **I. Introduction**

The literature informs us that there once was a “Chicago school” of economics, explicit references to which first appeared in the late 1940s (Medema 2018b).<sup>1</sup> The details, however, remain something of a mystery, and there is no settled agreement on the school’s origins, defining characteristics, or membership. This should not surprise, as drawing boundaries around “schools” of thought is notoriously difficult and fraught with perils. The search for and focus upon commonalities of method and outlook often masks significant heterogeneities and, in the process, gives rise to interpretive problems that impede understanding.

Ronald Coase is said to figure prominently in this “Chicago school, and that Coase took some small steps late in his career to distance himself from Chicago has done little to sever this link in the professional mind. The purpose of this paper is not to comb through Coase’s publications in order to discern whether he should, or should not, be appropriately considered a member of the “Chicago school.” Instead, it takes up Coase’s views on the Chicago school, as found in his published and, especially, unpublished writings, beginning already in the early 1960s, prior to his appointment at Chicago. These commentaries at once demonstrate a measure of kinship and significant differences of viewpoint, particularly as respects economic method. Pulling back the lens a bit further, the paper provides additional evidence for the heterogeneity of views on fundamental questions that existed even among ostensibly cornerstone members of the so-called “Chicago school.” In doing so, it highlights the ambiguity surrounding the idea of a “Chicago school” and adds credence to claims that there was no “true” Chicago or, what amounts to the same thing, that there were many Chicagos.

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<sup>1</sup>On the “Chicago school, see, e.g., Miller (1962), Reder (1982), Stigler (1988), van Overtveldt (2007), and Emmett (2010).

Why we should care about Coase's views of the Chicago school? Given the centrality ascribed to Chicago economics in general and the "Chicago school" in particular in the history of modern economics and economic policy making, it is important to come to grips with the basic contours of Chicago economics and the attitudes of its practitioners and, in the process, to begin to unpack whether the idea of a "Chicago school" is even a useful historical category. Looking at the subject from a slightly different angle, the history of the Chicago school is often discussed in the context of "movement history." As Ross Emmett has pointed out, this approach presents certain historiographic difficulties:

When this type of historiography takes over, ... the subtleties and nuances of real people disappear, as do their quests to understand and interpret ideas and events in new and/or different ways. Changes of mind, or new expressions of ideas, become irrelevant because everything about a person and her ideas is dictated by her place in the movement. (2018, 4)

This, in turn, leads Emmett to ask whether "telling stories about the individuals [would] undermine the story of the unified 'collective'" (2011, 135). The present paper attempts to shed light on this question and so contribute to Chicago-school historiography by moving the discussion to this "micro" level.

## **II. Dancing with Chicago**

What little formal education in economics Coase received came during his time in the B. Com. program at the London School of Economics (LSE) from 1929 – 1932, and this education was not devoid of Chicago influences and contacts. Coase read Frank Knight's *Risk, Uncertainty, and Profit* (1921) during his time there and, while traveling in the U.S. to explore the question of firm integration during his final year as an LSE student, visited the University of Chicago, meeting with Frank Knight and Jacob Viner (Coase 1988b, 14-16). Coase befriended Aaron Director when the latter spent a year at LSE in the late 1930s, and Director advocated for a

position for Coase at the University of Buffalo when Coase decided to emigrate to the US in the early 1950s.<sup>2</sup> Friedman, too, had by this point developed an appreciation for Coase and his work, particularly on the economics of broadcasting.<sup>3</sup> Even so, the evidence suggests that there was relatively little interaction between Coase and economists at Chicago until well into the 1950s.

That at least some economists at Chicago had an interest in Coase's work is evident from the fact that Coase's name was one of ten placed in nomination for a non-tenured appointment in the Department of Economics in 1951. No offer was forthcoming, however, and there is no evidence that Coase was actively considered for an appointment in the department in subsequent years.<sup>4</sup> That there was an affinity for Coase at Chicago, though, is evident from the Law School's first attempt to hire him, in the spring of 1956, likely at the instigation of Director. The Department of Economics was not a direct party to this hiring process, but T.W. Shultz, then chairman of the economics department, had written to Law School Dean Edward Levi to express "the Department's and his own pleasure in the appointment," noting that Milton Friedman considered Coase's recently published book on the British Broadcasting Company (1950), "something of a classic."<sup>5</sup>

Though Coase and Levi had agreed on salary and other terms, and Coase wrote to Levi accepting the offer on May 2, 1956, he did not, in the end, make the move to Chicago. The reasons for Coase's change of heart are unclear, but appear to have been salary- and teaching-related.<sup>6</sup> It is clear from his correspondence with Director, however, that Coase did not see the move as a huge attraction.<sup>7</sup> In fact, the prospect of having people like Director and Friedman as

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<sup>2</sup> See Aaron Director Papers, Box 1, Folder 2. Hereinafter, "Box-Folder" numbering references are used for archival materials unless otherwise noted.

<sup>3</sup> Friedman to Coase, December 7, 1948, Milton Friedman Papers, 23-28.

<sup>4</sup> The department unanimously voted to offer a contract to Arrow. See "Minutes: Meeting of Associate Professors and Full Professors," February 14, 1951, T.W. Schultz Papers, 41-2.

<sup>5</sup> Levi to Harrison, May 7, 1956. RHC 26-9. See also Levi to Coase, April 26, 1956, Ronald H Coase Papers (hereinafter, RHC) 26-9.

<sup>6</sup> Coase to Director, May 12, 1956, RHC 21-6.

<sup>7</sup> *Ibid.*

colleagues seems to have mattered little to Coase. (Stigler, whom Coase did not know well at this stage, had not yet arrived at Chicago.) What was of “decisive importance,” he wrote to Director ten days after initially accepting Levi’s offer, was his ability “to concentrate on the work I want to do,” work that he believed “ought to fit very well into a law and economics program.”<sup>8</sup> Absent such concerns, Coase said, “I have no doubt that it is in my own interest to come to Chicago.” But if the expected teaching obligations were not to his satisfaction, he believed that he was within his rights to withdraw from the agreement.<sup>9</sup> In the end, and for whatever reasons, the deal fell apart.

The bridges to Chicago clearly were not burned, however. Less than two years later, Friedman pushed Coase to apply for a fellowship at Stanford University’s Center for Advanced Study in the Behavioral Sciences, a fact that is noteworthy because Coase spent his year at the Center drafting his paper on “The Federal Communications Commission” (1959), which not long thereafter gave birth to “The Problem of Social Cost” (1960). In the interim, Coase accepted (after initially turning down) an offer to move from Buffalo to the University of Virginia, where he joined Chicago alumni including James Buchanan, Gordon Tullock, and Warren Nutter. Friedman, for his part, wrote that though he was happy to see Coase depart Buffalo for the “much more stimulating and exhilarating environment” of Virginia, he was “still most regretful not to have you around here.”<sup>10</sup> Even so, Coase’s correspondence with Buchanan finds him preoccupied with the same details—salary and benefits, and teaching burden<sup>11</sup>—that had concerned him when contemplating the move to Chicago, suggesting that these factors were far more important to Coase than the Virginia and Chicago intellectual environments.

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<sup>8</sup> *Ibid.* One must bear in mind when reading this statement that by “law and economics” Coase meant the study of the influence of law on the economic system, *not* the type of economic analysis of law that came to be associated with Richard Posner and others (including, wrongly, with Coase himself). See section IV.A, below.

<sup>9</sup> *Ibid.*

<sup>10</sup> Friedman to Coase, May 26, 1959, Milton Friedman Papers, 23.28.

<sup>11</sup> Coase to Buchanan, nd (1957 or 1958, likely spring 1958), RHC 19-7.

It was actually Chicago, rather than Coase, which instigated Coase's move to Chicago in 1964. Chicago approached him about a position in June, 1963, and Coase traveled to Chicago in August to discuss the possibilities.<sup>12</sup> The offer, which came in early December, was for a tenured full professorship in economics, rostered in the Law School and the Graduate School of Business and would make Coase a "full member" of the faculty in both schools. (The offer did *not*, it should be clear, involve an appointment in the Department of Economics.)<sup>13</sup> Coase responded immediately, noting that "The prospect of undertaking further work in the area of law and economics is naturally very exciting to me" and indicating his "determination to accept the offer" unless (as he thought unlikely) the particulars could not be resolved to the satisfaction of all parties. Still, Coase seemed to be in no hurry to get to Chicago, expressing a desire to avoid moving until the fall of 1965, or one year after the time contemplated by his prospective employers.<sup>14</sup>

Though Coase acceded to the request that he take up the position at Chicago for the fall of 1964, he continued to have qualms about the move, largely related to the *Journal of Law and Economics*, the editorship of which he considered the leading attraction of Chicago's offer. Coase had been informed by Harold Demsetz that, as Coase interpreted things, he would no longer be able to publish his own work in the *Journal* if he became its editor. This possibility was a potential deal-breaker for Coase given that the *Journal* "provides an outlet for the kind of article that I wish to write."<sup>15</sup> Law School Dean Phil Neal, though, was quick to allay Coase's fears, informing him that it would be "nonsense" to preclude an editor from contributing to his own journal and expressing the hope that Coase's writings "would be

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<sup>12</sup> This timeline is discussed in Coase's letter to UVA Dean Robert J. Harris, 11 March 1964, RHC 35-2.

<sup>13</sup> Interestingly, the offer almost did not come at all. Ed Levi, who had by this time risen to the position of Provost at Chicago, was adamantly opposed to Coase's appointment, owing to Coase's decision to back out of his 1956 commitment. It apparently was only due to Director's persuasion that Levi relented and allowed the hiring process to go forward. Ning Wang to the author, June 7, 2018. Ning Wang was Coase's student, friend, and co-author. See also Shultz to Coase, December 13, 1963, RHC 32-13.

<sup>14</sup> Coase to Neal, December 9, 1963, RHC 30-9.

<sup>15</sup> Coase to Neal, January 11, 1964, RHC 30-9.



conspicuous” in the journal.<sup>16</sup> On February 2, 1964, Coase wrote to Neal to formally accept Chicago’s offer.

Yet another potential hurdle to Coase’s move arose some two months later. The archival details here are sketchy, Coase’s side of the correspondence not having been preserved, but Coase’s concern appears to have been that the *Journal of Law and Economics* might be viewed, and used, as a “‘house’ organ” (Neal’s words).<sup>17</sup> What we do know is that the *Journal* was, at this time, nearly moribund, leaving Friedman “dubious” that it could be “saved.”<sup>18</sup> An exchange of correspondence between Coase and Stigler (only Stigler’s side being preserved) indicates that the *Journal* was both critically short of articles—leading Stigler to solicit papers from his Chicago colleagues to prop it up—and two years behind schedule.<sup>19</sup> The “house organ” issue was by no means a new one; the *Journal* had great difficulty in attracting submissions from the start, and most of the articles published in it were written by Chicago faculty or alumni, and their associates. This turn in the conversation suggests that Coase had not been aware of the *Journal*’s struggles, causing him to question whether he should, in fact, make the leap to Chicago. Neal again quickly sought to set Coase at ease, emphasizing that Coase would have a free hand and the funding to publish articles that he believed suitable for the *Journal*,<sup>20</sup> and that there was “no thought of trying to make the JLE a ‘house’ organ.”<sup>21</sup> Given that Coase went forward with the move to Chicago, one must assume that he was satisfied with Neal’s response.

In the end, however, Coase’s move to Chicago owed as much to the indifference of the

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<sup>16</sup> Neal to Coase, January 15, 1964, RHC 30-9.

<sup>17</sup> Neal to Coase, April 13, 1964. RHC 30-9.

<sup>18</sup> Friedman to Stigler, February 6, 1963, Stigler Papers, Box 24, Folder of “Correspondence involving the article ‘Roofs or Ceilings’ by G.J. Stigler and M. Friedman, incl. early drafts, related corresp, correspondence w. Milton.”

<sup>19</sup> Stigler to Coase, January 27, 1964 and February 4, 1964, RHC 33-6.

<sup>20</sup> Neal to Coase, April 13, 1964. RHC 30-9.

<sup>21</sup> *Ibid.*

Virginia administration as to what was on offer on the Midway.<sup>22</sup> Though the administration had known of Chicago's interest in Coase for many months, they responded with silence, leading Coase—who had turned down feelers from Harvard only a year earlier because of his desire to remain at Virginia—to explore the possibilities at Chicago. Dean Robert Harris finally agreed to meet with Coase on January 14, 1964, but made no attempt to retain his services. Harris was highly critical of the department's perceived lack of diversity of intellectual points of view and recommended a major reorientation, precipitating the departures of Tullock and Buchanan as well. Coase seems to finger Harris's distaste for the department directly in his resignation letter, informing him that, unlike at Virginia, "The authorities of the University of Chicago have left me in no doubt of their devotion to scholarship and of their determination to provide an environment in which scholarly work is encouraged and assisted in every way possible." Had such encouragement been present at Virginia—and the blow-up there made clear that it was not—there is good reason to suppose that Coase would have remained in Charlottesville.

### **III: On Chicago, Before Chicago**

Unlike Stigler, then, Coase does not seem to have been pining away to get to Chicago. That Coase did not see a strong affinity between himself and the economists at Chicago is evident from his response to a suggestion, made before he arrived at Chicago in 1964, that he was, in fact, a member of the "Chicago school."

#### *A. Against Miller*

In 1962, UCLA's H. Laurence Miller published a paper entitled, "On the Chicago School of Economics" in the *Journal of Political Economy (JPE)* which attempted to identify "the set of common attitudes and interests which distinguishes [Chicago school economists] from the rest

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<sup>22</sup> The information that follows comes from Coase's resignation letter, written to Dean Robert J. Harris on 11 March 1964, RHC 35-2. See Breit (1987) and Medema (2000) for discussions of the larger blow-up at Virginia.

of the profession” (1962, 64). Among the eight economists Miller explicitly identified with this “school” was Ronald Coase, who at this point had no affiliation with Chicago.<sup>23</sup>

Miller argued that “the Chicago economist” is distinguished from other economists by “a number of closely related attributes”:

the polar position that he occupies among economists as an advocate of an individualistic market economy; the emphasis that he puts on the usefulness and relevance of neo-neoclassical economic theory; the way in which he equates the actual and the ideal market; the way in which he sees and applies economics in and to every nook and cranny of life; and the emphasis that he puts on hypothesis-testing as a neglected element in the development of positive economics. (1962, 65)

Coase entered the analysis when Miller discussed how the “Advocacy of an individualistic market economy appears throughout Chicago economists’ work,” including via the quest for “new ways to introduce the market system of rewards and penalties” (66). Coase’s (1959) analysis of the possibilities of assigning private property rights in broadcast frequencies was, for Miller (1962, 66n.10), exemplary on this score.

Miller’s characterization of the Chicago school was challenged by Stigler (1962) and Martin Bronfenbrenner (1962). They questioned the very idea of a Chicago school and the accuracy of Miller’s generalizations, with Stigler suggesting that it was silly to identify Coase with Chicago given that he had never been on the Chicago faculty. Coase, too, drafted a response to Miller’s article—one that was never published but which finds him at once denying the existence of a Chicago school, defending certain aspects of the Chicago approach, and attempting to separate himself from Chicago.<sup>24</sup>

Coase considered “absurd” the entire notion of a “Chicago school” of economics. The

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<sup>23</sup> The only other individuals mentioned by Miller were Knight, Viner, Simons, Friedman, Stigler, Reuben Kessel, and Simon Rottenberg.

<sup>24</sup> See Coase, “The Chicago School of Economics,” nd 1963, RHC 58-3 (hereinafter, Coase, “Chicago School”). The opening of Coase’s text suggests that it was read out at a seminar on the “Chicago School of Economics,” likely held at the University of Virginia.

problem, he said, “is that [the existence of a Chicago school] *is* taken seriously (by economists all over the United States).”<sup>25</sup> Coase did allow that Chicago had on its faculty, both at present and in the past, economists of “high calibre,” but he was of the mind that they had not, like Smith, Marshall, and Keynes, “changed the character of our subject—so that we will never look at economic problems in quite the same way again.”<sup>26</sup> Chicago was not, it would seem, a city on a hill so far as Coase was concerned.

Coase clearly felt no sense of connection to a “Chicago school,” saying of his own reaction to the association made by Miller that “John F. Kennedy, finding himself described as a member of the Church of England, could not have been more surprised.”<sup>27</sup> Coase reminded his audience that he had neither studied nor taught at Chicago, and that LSE, where he *had* both studied and taught, had provided the main influence on his thinking. Nor did he welcome the association, accusing Miller of “associat[ing] my name with a travesty of my views.” Coase acknowledged that he had suggested that the market be considered as a mechanism for allocating broadcast frequencies, but pointed to statements he had made in the FCC paper and elsewhere (e.g., Coase 1937) to the effect that the market is *not* always the best mechanism for allocating resources.

It is noteworthy that, though Coase found Miller’s depiction of the characteristics of the Chicago school “unconvincing,” he did not go so far as to suggest that the zeal for market organization was an erroneous depiction—except as applied to his own case. But he felt that Chicago economics was getting a bad rap within and beyond the profession for the position of some of its faculty on the role of government in the economy. He attributed this to the dominant left-wing attitude within the economics professoriate, where anyone “moderately skeptical” of more government intervention was considered “extreme.” “Chicago school,” Coase believed, had become a term of opprobrium, used to attack anyone or any ideas that

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<sup>25</sup> Coase, “Chicago School,” 1.

<sup>26</sup> *Ibid.*, 2-3.

<sup>27</sup> Kennedy was, of course, noted (and controversial) for being the first Roman Catholic President of the United States.

disputed the efficacy of governmental controls, regardless of any actual connection to Chicago.

Coase also took issue with Miller's (1963, 67) argument that Chicago economists have a "tendency ... to emphasize the usefulness and relevance of neo-neoclassical economic theory" and downplay "the troublesome implications of oligopoly and unions for competitive theory" (1962, 67). In Coase's opinion, Miller was doing nothing more here than criticizing Chicago's Marshallianism, a trait which did little more than illustrate Chicago economists' "good sense." Coase's attitude here should come as no surprise when one recognizes his fondness for Marshall's economics (Medema 2018a), but Coase also confessed that he found it "odd that admirers of Alfred Marshall should be described as members of the Chicago School," saying that "It is rather like describing admirers of Karl Marx as members of the Harvard School."<sup>28</sup>

### *B. Against Friedman*

Having challenged both the idea of a Chicago school and the veracity of Miller's attempt to locate him within it, Coase turned, at the end of his narrative, to his own views on one specific aspect of Chicago thinking that he considered distinctive and "of some importance"—the methodological stance taken by Friedman in his famous essay on "The Methodology of Positive Economics" (1953), to which Stigler, as Coase noted, also subscribed.<sup>29</sup> Coase had first been exposed to Friedman's methodological position when he attended a talk that Friedman gave in London prior to the publication of the methodology essay. He reports that his "immediate response was unfavorable" (1982b, 15),<sup>30</sup> and his 1963 comments made clear that the intervening decade had not altered his views on the subject.

Coase summarized his interpretation of Friedman's methodological approach as follows: "You adopt a theory not on the basis of the realism of its assumptions but if it gives

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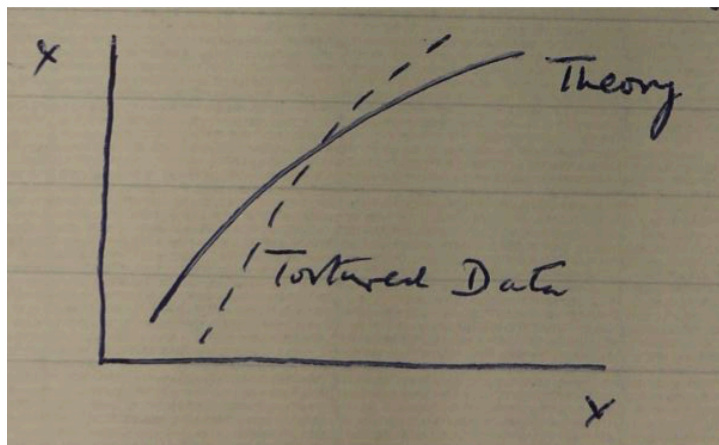
<sup>28</sup> Coase, "Chicago School," 5.

<sup>29</sup> Coase, "Chicago School," 5. Stigler came to this position only after much initial resistance. See, e.g., Hammond (2009).

<sup>30</sup> Coase also noted that he challenged Friedman at the time, but an unbiased observer would have concluded that he "lost every round" (1982b, 15).

predictions that are valid.” Quantitative empirical analysis was thus brought to the fore. Though virtually all of Coase’s empirical work was of the *qualitative* kind—pouring through government documents, legal cases, and the like—he was not averse to quantitative analysis. His work on producer expectations (Coase and Fowler 1940) was both overtly quantitative and “greatly influenced by the work of Henry Schultz of the University of Chicago in deriving statistical demand schedules” (Coase 2004, 196). That work has led Van Overtveldt to claim that Coase “was an early subscriber to a basic ingredient of the Chicago Tradition during the Friedman era—that theoretical developments have to be backed up by empirical evidence” (2007, 203). This, however, represents a serious misreading of Coase’s position. Coase was clearly averse to modern statistical techniques and the conclusions that economists drew from them. As he said in a 2010 interview, “A regression with aggregated statistical data will not tell you much about the way the economy works” (Wang 2014, 107).<sup>31</sup> And as his 1962 commentary makes clear, he also had serious issues with Friedman’s view of the relationship between theory and empirical work.

Figure 1<sup>32</sup>



For Coase, Friedman’s approach was wrongheaded, and for at least two reasons. First, he said, “All hypotheses are likely to be confirmed (partic [sic] after you have finished

<sup>31</sup> As Reder (1982, 6n.14) has pointed out, Coase’s views on this score can be described as more akin to Knight’s and Buchanan’s than to Friedman’s or Stigler’s.

<sup>32</sup> Coase, “Chicago School,” 6.

torturing the data). Nature may be reluctant to confess—but it confesses in the end.”<sup>33</sup> But this, for Coase, did not resolve the validation issue. Referring to his hand-drawn figure, reproduced above, comparing a theory’s predictions for the relationship between  $x$  and  $y$  with what the data reveals, Coase asked, “Is this this validation? Should you adopt the theory?” The answer, he said, is “not clear,” since “what are grounds for acceptance for one person may be grounds for rejection by another.”<sup>34</sup> Against Friedman, Coase maintained that it is essential to think about the problem, and to judge a theory, realistically. Friedman’s approach, he said, misses out on the true purpose of theory, that being “to give you an insight into what is going on—to give you understanding—to give you a base for thought.” The fact that economists are interested in “the real economic system” means that “we are interested in our theory having realistic assumptions.” If a single firm acts as if it has 10,000 competitors, Coase argued, then “we should not ignore this fact” but instead work to develop a theory of why the single firm behaves in this fashion. “[U]ntil we do,” he continued, “we won’t know how to think about this firm.”<sup>35</sup> The Chicago view of the general applicability of the competitive model, then, held little sway with Coase (2014, 77).

But there was a second aspect to Coase’s criticism. Friedman’s “heavy emphasis on hypothesis testing,” Coase argued, offered “a misleading account of the scientific process.”<sup>36</sup> In reality, he said, “It is much nearer to the truth to say that we test a theory because we believe in it than we believe a theory because we have tested it.”<sup>37</sup> Indeed, rather than waiting for empirical validation, economists use a theory “because it fits in with what we know and enables us to think about the problems,” and assume that validation will ultimately come. And

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<sup>33</sup> Coase, “Chicago School,” 6. Coase later modified this to his now well-known statement that, “if you torture the data enough, nature will always confess” (Coase 1982b, 16). Coase’s 1963 comment may have been influenced by Kuhn’s (1961, 176) contention that “nature undoubtedly responds to the theoretical predispositions with which she is approached by the measuring scientist.” See the discussion in section IV.B, below.

<sup>34</sup> Coase, “Chicago School,” 6.

<sup>35</sup> *Ibid.*

<sup>36</sup> *Ibid.*, 8.

<sup>37</sup> *Ibid.*, 7-8.

even when the data fails to validate the theory, Coase claimed, economists typically continue to utilize that theory until one offering better insights becomes available.

Coase considered all of this more than a minor methodological quibble, emphasizing that “*if this is what the Chicago School thinks, I am opposed to it.*”<sup>38</sup> This was the first time that Coase had publicly set himself in opposition to a foundational element of what came to be known as the Chicago view, but it certainly was not the last. It is important to bear in mind that Coase said all of this in 1962, some two years before he arrived at Chicago. The reality, though, is that his roughly two decades on the Chicago faculty did little to change his views and, in fact, may have increased in the distance that he perceived between his own approach and that of the evolving Chicago school.

#### **IV. In Chicago, But Not of Chicago**

One may be tempted to think that, when Coase arrived at Chicago, he was finally “home” and among friends with whom he would work to reshape modern economics in the Chicago image. But this is to misapprehend the history. Coase and Friedman had a friendly personal relationship, but their interaction after Coase’s arrival seems not to have been terribly extensive—not surprising, given their divergent research interests. Coase also had a warm relationship with Director, but Director decamped to California and the Hoover Institution soon after Coase arrived at Chicago. Interestingly, it was Stigler, who Coase (1991, 469) “did not know ... at all well” before coming to Chicago, to whom he became particularly close, even if the passing of time did put some distance between them.<sup>39</sup> And while Coase did share with his illustrious Chicago colleagues the goal of transforming economics, their views of what that transformation should look like were very different.

##### *A. What is Economics?*

Coase’s long association with the Posner- and Becker-inspired economic analysis of law belies

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<sup>38</sup> *Ibid.*, 8, emphasis added.

<sup>39</sup> See section IV.D, below.



a simple truth: He had no interest at all in this field, the genesis of which saw modern Chicago at the tip of the spear.<sup>40</sup> Indeed, Coase had a particularly dim view of economists' efforts to expand the boundaries of economics beyond their traditional subject-matter confines, a topic to which he referred on multiple occasions but which received extensive play in his article, "Economics and Contiguous Disciplines" (1978).<sup>41</sup>

The subject of Coase's essay was "what determines the boundaries between disciplines," in particular, between economics and the other social sciences (1978, 201). No stranger to the sociology of science, Coase reported that he had long been content with the definition of economics ascribed to Jacob Viner, that "economics is what economists do," the boundaries around which, he suggested, are "determined by competition":

The practitioners in a given discipline extend or narrow the range of the questions that they attempt to answer according to whether they find it profitable to do so, and this is determined, in part, by the success or failure of the practitioners in other disciplines in answering the same questions. (202)

At that time, of course, economists had for more than a decade been steadily expanding the boundaries of their analysis into turf traditionally belonging to other social sciences, an effort spearheaded by Gary Becker, George Stigler, Richard Posner, and other of Coase's Chicago colleagues. The question, for Coase, was whether these efforts would be successful in the long run.

Coase identified three forces that can bind together a group of scholars into a separate profession: "common techniques of analysis, a common theory or approach to the subject, or a common subject matter" (204). Coase left no uncertainty about where he stood on this matter, that "in the long run it is the subject matter, the kind of question which the practitioners are trying to answer, which tends to be the dominant factor producing the cohesive force that

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<sup>40</sup> See, e.g., Coase (1993, 521) and his comments in Kitch (1983, 192), as well as Posner (1993a,b). On Coase's vision for "law and economics," see below.

<sup>41</sup> This paper was originally published as Coase (1977a).

makes a group of scholars a recognizable profession with its own university departments, journals, and libraries“ (204). This view, Coase recognized, put him squarely at odds with Becker, who believed that economists were defined by their “approach” and that this approach would allow economists to be successful contributors to and perhaps even displace practitioners in these other fields.

Becker’s vision, as Coase noted, was grounded in Robbins’ definition of economics, which made economics “a study of human choice” and so “coterminous with all of the social sciences” (207). Coase’s own preference, however, was for the subject-matter based definitions of the discipline, such as those found in Marshall (1920) or in early editions of Stigler’s price theory text (e.g., 1952, 1).<sup>42</sup> Coase allowed that technique or approach may matter in the short run, but he had little confidence that it would present any advantage for the economist in addressing problems within these other fields in the long run, and for two reasons. First, if the economic approach proved fruitful in other disciplines, scholars in those disciplines would adopt this approach themselves. And because economists lacked the subject-matter knowledge to be effective core contributors to these fields, they would not be able to compete effectively against that field’s professionals in the marketplace of ideas (204-205).

Coase was skeptical, however, that the economic approach would ultimately shed important new light on these other fields. At the center of the Becker-Posner approach, Coase said, is the treatment of the individual as a “rational, utility-maximizer” (208). But Coase was not convinced that utility theory offered a fruitful framework for analyzing individual behavior in other social science contexts. In fact, he was not at all convinced that utility theory was useful for *economic* analysis, calling its role in the discipline “largely sterile,” in that “it tells us nothing about the purposes for which [people] engage in economic activity and leaves us without any insight into why people do what they do” (208). As a result, he said, “Utility theory seems more likely to handicap than to aid economists in their work in contiguous

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<sup>42</sup> Of course, by 1977 Stigler had joined forces with Becker in pushing the boundaries outward, a fact that Coase elected not to note in his paper.

disciplines” (208). Coase found Adam Smith’s view of man, as expressed in *The Theory of Moral Sentiments* and *The Wealth of Nations*, much more congenial and in this, too, differed from Chicago colleagues such as Stigler and Friedman, who saw a modern economist in this as in so many other aspects of Smith.<sup>43</sup>

Coase’s decision to immediately reprint “Economics and Contiguous Disciplines” in the *Journal of Law and Economics* undoubtedly had a message behind it, perhaps not unlike Harry Johnson’s decision to publish Miller’s article—which he had to know would offend several of his Chicago colleagues—in the *JPE*. Coase understood full well that the volume in which the paper was originally published, targeted at *librarians*, was unlikely to garner a wide readership among economists, and he was keen to make his views on the subject more broadly known. All of this also helps us to understand why, when Coase informed Director in 1956 that he (Coase) “ought to fit very well into a law and economics program,”<sup>44</sup> he was not portending modern Chicago economic analysis of law. Instead, he was speaking to his own vision for “law and economics”: the examination of how legal rules influence the economic system, the study of which he believed would improve economics and a feature of Adam Smith’s approach to economic that Coase greatly appreciated. This was the vision he brought to the editorship of the *Journal of Law and Economics*, believing it could serve as an outlet to encourage this type of scholarship. That Coase had little interest in the direction taken by law and economics at Chicago during the latter part of the century is evidenced in his hope that Demsetz, who shared his vision for law and economics, would return to Chicago from UCLA to take over the Law School’s Law and Economics program. That Chicago was uninterested in someone who did not possess a law degree—and in Demsetz in particular—and that Coase would prefer Demsetz for this position at all, speaks to how far removed the Chicago program had become from Coase’s original vision.<sup>45</sup>

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<sup>43</sup> See Coase (1976; 1977b), Stigler (1976), and Friedman (1977).

<sup>44</sup> Coase to Director, May 12, 1956, RHC 21-6.

<sup>45</sup> Demsetz to Coase, April 11, 1991, RHC 21-4.

### *B. Friedman Redux*

Coase returned to the subject of Friedman's methodological precepts in his 1981 G. Warren Nutter Lecture at the American Enterprise Institute. The lecture's title, "How Should Economists Choose?" (1982b) was pointed squarely at Friedman, and the text of the lecture, which expanded significantly on his 1963 commentary, makes clear that Coase had not modified his views, even though Coase counted himself among Friedman's "friends and admirers" and offered profuse praise to Friedman upon the latter's receipt of the Nobel Prize in 1976.<sup>46</sup> What differed this time around is that Friedman responded—pointedly and at length—to Coase's arguments.

Realism in theory construction remained as central for Coase in 1982 as it had been in 1963. "Faced with a choice between a theory which predicts well but gives us little insight into how the [economic] system works and one which gives us this insight but predicts badly," he said, "I would choose the latter," and he was convinced that "most economists" would do the same (6). The extent to which Coase believed that his views on realism separated him from the "Chicago school" view is evident from his 1983 letter to Yale's George Priest:

I think you are right in saying that my way of looking at things is not the same as that of the other Chicago economists. The difference, I think, is that they are not interested in the effect of positive transaction costs on what the institutional structure is and should be (or not very interested) and in the main confine their analysis to problems which can be tackled with the ordinary tools of economic analysis. When Reuben Kessel wrote his first draft on Blood and the Coase Theorem, I remember being astonished to discover that he had never read the later sections of the Social Cost article dealing with the effects of positive transaction costs.<sup>47</sup>

Coase's comments here, of course, go directly (though not exclusively) to what he perceived as

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<sup>46</sup> Coase to Friedman, October 16, 1976, Friedman Papers 23-28.

<sup>47</sup> Coase to Priest, with attached comments, January 26, 1983, RHC 31-12. A similar sentiment can be found in Coase (2014, 77).

Chicago's wrong-headed fixation on the so-called "Coase theorem." Friedman, famously, had led the attack against Coase's negotiation result during the famous salon at Aaron Director's home circa 1960. It was Stigler who, not long thereafter, had elevated Coase's negotiation result to a "theorem" and, for Coase, had derailed the profession from the larger message of his article on social cost (Medema 2011). That Reder (1982, 22) (accurately) described the Coase theorem, the genesis of which Coase came to regret, as a "Chicago-type innovation" only serves to emphasize Coase's distance from Chicago and the Friedman–Stigler approach to theorizing.

Coase also continued to emphasize how far professional practice, in his mind at least, deviated from Friedman's prescription, pointing to the quick acceptance accorded the market structure theories of Joan Robinson and Edward Chamberlin and, soon after, Keynes's *General Theory* absent any significant predictive evidence. Instead, these theories were accepted because they "seemed to give us a better understanding" of the economy's workings (1982b, 12). What role, then, did quantitative analysis play in the choice among theories? The answer, for Coase, lay in the work of Thomas Kuhn, with whom he spent the 1958-59 academic year at Stanford's Center for Advanced Study in the Behavioral Sciences (15-16). Two aspects of Kuhn's argument particularly resonated with Coase. The first was his insight about the tight relationship between the scientist's "theoretical predispositions" and the results of his measurement activity, which he had brought out (without naming Kuhn) in his 1963 discussion (1982b, 16, quoting Kuhn 1961, 200). But Coase was also impressed by Kuhn's related claim about the persuasive function served by empirical testing. In the process of choosing between competing theories, Coase said, quantitative studies

perform a function similar to that of advertising and other promotional activities in the normal products market. They do not aim simply at enlarging the understanding of those who believe in the theory but also at attracting those who do not believe in it and at preventing the defection of existing believers. These studies demonstrate the power

of the theory, and the definiteness of quantitative studies enables them to make their point in a particularly persuasive form. What we are dealing with is a competitive process in which purveyors of the various theories attempt to sell their wares. (17)<sup>48</sup>

Economists who choose a theory because of the “base for thinking” it provides will not inevitably subscribe to the same theories, he said. “They may be interested in different problems or approach the same problem in rather different ways or use different techniques of analysis, and these factors may lead them to prefer one theory rather than another.” Invoking a piece of Friedman’s oeuvre of which he was more fond, Coase argued that, “In such cases, there is little that should be done other than to leave economists free to choose” (Coase 1982b, 18).<sup>49</sup>

### *C. Methodology v. the Sociology of Science*

Coase’s Nutter lecture did not go unchallenged by Friedman, who responded to Coase’s discussion with a four-page typed letter in early 1982. To say that Friedman was aggrieved would be something of an understatement and he, like Coase, seems not to have forgotten their initial exchanges on the subject in the early 1950s: “I fear that we are where we were when we first discussed this many years ago, talking at cross-purposes. As I read your paper, I believe that you are not criticizing the paper I wrote but the paper you read and any relation between the two seems to be wholly coincidental.”<sup>50</sup> It also seems clear that Friedman considered Coase’s lecture an attack on something larger than one of his writings—that is, on the Chicago way of doing things—as he copied his letter to Stigler, Director, and Becker. Friedman then proceeded to offer an almost point-by-point refutation of the arguments that Coase had

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<sup>48</sup> Coase found support for this position from within the Chicago tradition, but a slice of it hostile to the “Chicago school”—Don Patinkin. See Patinkin (1972, 142).

<sup>49</sup> Coase was obviously thinking back to the blow-up at Virginia when he remarked, in this vein, that “A belief that the empirical findings by research workers in all economics departments should be the same might lead an arrogant and ignorant university administration to attempt to destroy an economics department that had a distinctive character and to attempt to remake it so as to be like Yale (few would want all economics departments to be like Chicago). But that would be the way to mediocrity for that university as well as impeding the search for truth by restraining the competitive process.” (18)

<sup>50</sup> Friedman to Coase, January 7, 1982, RHC 22-12.

presented in his lecture, on multiple occasions dismissing Coase's criticisms as "debating points" rather than "serious argument[s]." We can do no more than to summarize Friedman's reactions here.

Friedman disputed Coase's contention that there can be a theory that predicts badly but yet generates insight and questioned the very idea of being able to discriminate between more and less realistic assumptions. He was convinced that, in rejecting the prediction criterion, Coase had left himself—and economists generally—with no means for choosing among alternative theories, a task which they both agreed (and this was perhaps their only point of agreement) was at the heart of the matter.<sup>51</sup> Friedman's most vehement reaction, though was reserved for Coase's suggestion that he was attempting to lay out a positive theory of how economists choose the theories they espouse, accusing Coase of "setting up a red herring." Even so, Friedman rejected Coase's contention that the acceptance of the theories of Robinson, Chamberlin, and Keynes occurred absent empirical testing. The old theories were rejected and the new ones accepted, he said, precisely because the predictions of the new theories, whether based on quantitative analysis or observation of the world around us, "seemed to conform better to the observed phenomena than did the [old] paradigm."<sup>52</sup>

Coase's response to Friedman, also copied to Stigler, Director and Becker, was, at five typed pages, even longer than Friedman's original.<sup>53</sup> Coase continued to defend his position on the distinction between "insight" and "prediction" and, in doing so, got to the crux of his disagreement with Friedman. For Friedman, theory was a vehicle for generating predictions about economic activity. For Coase, though, it was "*a logical structure*," constructed to provide insight into the real-world economic system. Unlike a theory that may predict well but is based on highly unrealistic assumptions, he said, a theory that generates insight "enables one to see how the factors at work in the economic system are interrelated, how a change in one

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<sup>51</sup> *Ibid.*

<sup>52</sup> *Ibid.*

<sup>53</sup> Coase to Friedman, February 22, 1982, RHC 22-12.

factor affects others and we are enabled, by a logical argument, to go from assumptions which are really in the category of the self-evident to conclusions which are anything but self-evident.” And because the results are derived from almost self-evident assumptions, he continued, “one can have a great deal of confidence in these results (or, at any rate, economists do).” This view of theory necessitates careful attention to realism in one’s assumptions and here, Coase said, the test is basically an economic one: “If a factor is handleable, relevant and its inclusion costs less than you gain, you include it, otherwise you do not.”<sup>54</sup>

Coase emphasized that he had “little interest” in methodological strictures such as those offered by Friedman—or anyone else.<sup>55</sup> Why was Coase so opposed to methodology and, specifically, to Friedman’s approach to theory choice? The answers are two and, ironically, decidedly Friedmanian. First, Coase believed that the most useful approach to the methodological problem is to transform it into an economic problem, a position with which he had thought Friedman would surely agree. Second, Coase’s preferred solution to the economic problem invoked Friedman himself, though the Friedman of *Capitalism and Freedom* or *Free to Choose* rather than Friedman (1953)—that economists “should be free to choose between the alternatives offered to them” and that they “can be counted upon to choose those theories which enable them to make their most valuable contribution.”<sup>56</sup>

This conversation between Friedman and Coase came to a close with Friedman’s relatively brief letter to Coase of April 19, 1982. Coase’s February letter, Friedman said, made it clear to him that they were “talking completely at cross-purposes,” with Coase seeing the sociology of science as “the only legitimate subject for investigation” and, from Friedman’s perspective, offering no means of choosing between theories “except verbal argument and ultimately conflict.”<sup>57</sup> In closing his letter, Friedman shed indirect light on Coase’s uneasy relationship with Chicago, saying,

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<sup>54</sup> *Ibid.*, emphasis added.

<sup>55</sup> *Ibid.*

<sup>56</sup> *Ibid.*

<sup>57</sup> Friedman to Coase, April 19, 1982, RHC 22-12.



In view of the difference between us about the kind of writing and work we regard as legitimate, I fear there is little to be gained by continuing the discussion on the levels on which we have been carrying it out. When we treat common problems with a common objective we find ourselves in almost complete agreement. But that is not a guarantee that that agreement will carry over to other areas. Hence, it should not be surprising if we speak at cross-purposes in some other area.<sup>58</sup>

No guarantee, indeed, given that the space over which Coase found himself at odds with his Chicago colleagues went well beyond the particulars of his debate with Friedman.

*D. For, and Against, Stigler*

It was Knight and Stigler whom Coase (2004) most admired among his Chicago colleagues, but as Stigler's economics moved progressively away from Knight and toward Friedman (methodologically) and Becker (theoretically), the links between Coase and "Chicago" became even more frayed. Coase seems to have been less disturbed about Friedman's own position than its influence on Stigler, and he felt that the direction of influence in the Stigler–Becker collaboration ran from the latter to the former. "I don't know always to [Stigler's] advantage," Coase said, "but that's what happened" (Freedman 1997, 4).

Coase saw Stigler, as well as his Law School colleague Richard Posner, as prototypical examples of what he had learned from Kuhn:

It seems to me that when you get to [Stigler's] later work, say with Becker, you know what the conclusion is going to be before you start the argument. In a sense, you're assembling arguments to support a conclusion. I mean, that may be unkind and untrue but it's an impression. And, it's even more so in the work of Richard Posner. Have you read any of that? It seems to me that the plot is always the same, and the characters stay fixed.<sup>59</sup>

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<sup>58</sup> *Ibid.*

<sup>59</sup> Freedman (1997, 7). Curiously, Coase says the same about Becker but with a more charitable end view: "It's the same thing, to some extent, one might say about Becker. But his work is so very good. And you learn so much from studying it, that that element in it is not a problem" (*ibid.*).

Stigler, he said, “knew what the answer was going to be” before he started his empirical analysis. “He just regarded it, as I say, as a way of persuading other people” (Freedman 1997, 9), as ammunition in his efforts “to change the world” (14). It is perhaps no accident, then, that Coase found Stigler to be “at his best” in his early-career work in the history of economic thought (1982a, 21).<sup>60</sup>

Nonetheless, Coase was effusive in his praise for Stigler in the two essays that he wrote on Stigler’s contributions, and he offered a moving tribute to Stigler, who had passed away only days before, as a prelude to his own Nobel lecture in Stockholm.<sup>61</sup> Coase declared himself “fascinated” by Stigler’s mind (1991, 469), calling him “a man *sui generis*” and saying that “Stigler never deals with a subject which he does not illuminate” (1982a, 21). But the relationship went beyond scholarly admiration. Stigler, said Coase, “was always very nice and kind and helpful in many ways. Always.” He was an early champion of Coase’s long-neglected article, “The Nature of the Firm” (1937), reprinting it in the AEA’s *Readings in Price Theory* (1952),<sup>62</sup> and of “The Problem of Social Cost.” He threw his weight behind Coase’s appointment to the Graduate School of Business at Chicago,<sup>63</sup> gave Coase funding to cover what would otherwise have been an unpaid leave term during his first year at Chicago,<sup>64</sup> lent significant assistance to Coase’s efforts to improve the *Journal of Law and Economics*, and nominated Coase for the Nobel Prize at least as early as 1987. Their respective archives document the extensive correspondence between them and the enthusiasm that each had for the person and work of the other.

Even with all of that support, however, Coase said, late in life, “I often wondered how

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<sup>60</sup> See also Coase (1991). Posner (1993b, 199) later chided Coase for this assessment of Stigler’s contributions.

<sup>61</sup> See “A tribute to George Stigler before I delivered my Nobel Memorial Lecture, December 9, 1991, in Stockholm,” RHC 50-3.

<sup>62</sup> This made Stigler one of the few people to take serious note of the article prior to the 1970s. See Coase (1988a).

<sup>63</sup> Harold Demsetz and George Stigler, “Report of Committee on Ronald Coase,” nd (1963 or 1964), Stigler Papers, Addenda, Box 7.

<sup>64</sup> Stigler to Coase, February 19, 1964, RHC 30-9.

far [Stigler] agreed with what I was saying. I think he thought I was all right, but a little odd” (Freedman 1997, 14). Though Stigler never offered Coase his opinion of Coase’s critique of Friedman’s methodology (7), he was anything but pleased with Coase’s essay on “Economics and Contiguous Disciplines.” His response to the paper, which was originally presented at a conference in Germany, was that “it would have been better if it had been written in German” (4). But while Stigler could be unsparing to those with whom he disagreed, he seems to have been content to turn Coase’s idiosyncrasies into a positive for Chicago, remarking that Coase’s opposition to “both the heavy emphasis on quantification and the bolder extensions of price theory to other social sciences” speaks to “the variety of views held by members of the modern Chicago School.”<sup>65</sup>

## **V. Conclusion: Chicago Economics and “Good” Economics**

In an essay on Marshall’s methodology, Coase remarked that

We are inclined to think of the Cambridge economists working together at Cambridge in the period before the publication of Alfred Marshall’s *Principles of Economics* as a “little band of brothers” ... In fact, this picture is incorrect. They were not a “little band of brothers.” They did not have a common view. ... All of which suggests certain lack of sympathy with Marshall, not only with his manners *but with his aims*. And we know from other evidence that this inference is correct.” (Coase 1975, 25, emphasis added)

It is hard to imagine that Coase did not have the “Chicago school” somewhere in the back of his mind when penning these words. Even so, it is difficult to know to what extent Coase would apply this statement across others identified with the Chicago school. What is unmistakable, though, is that he did not self-identify with a Chicago school, even as other of its ostensible members reveled in it (Friedman 1974; Stigler 1988). It is common to find differing points of view among members of so-called “schools,” but Coase’s disagreements

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<sup>65</sup> Stigler, “The Chicago School of Economics,” manuscript, September 27, 1979, Stigler Papers, Box 20.

with and divergences from Chicago were deeper and more fundamental than those between others identified with the Chicago school, particularly on the methodological (broadly defined) front. One can certainly find commonalities, particularly as regards his dim view of the possibilities of state action as against the market process. Even here, however, Coase minimized the Chicago influence. Having been steeped in “the benefits which flow from an economy directed by the pricing system” at LSE, Coase could state in a 1988 retrospective that, “Clearly, I did not need Chicago” (1988b, 7). But in what are often considered the key analytical identifiers of post-WWII Chicago economics—Friedman’s methodological precepts, the model of perfect competition, the definition of economics, and the interest in pushing the boundaries of economics—Coase consciously distanced himself from Chicago and his colleagues there. He was, as Lester Telser put it, an “independent thinker,”<sup>66</sup> and to the extent that he did identify himself with a “school-like” entity, it was with LSE, not Chicago.

Coase’s commentaries about moving to Chicago, both at the time and in later years, make clear his belief that this move would allow him to do the type of work that *he* wanted to do, not that he would be joining a merry band of fellow travelers. It should not be missed that the *Law School* was the environment in which he could undertake the sort of institutional structures of regulatory bodies and government enterprises that dominated his research agenda during the post-WWII period.<sup>67</sup> The economics department, it would seem, had no real interest in bringing on board someone with Coase’s particular interests and approach. Coase admitted that this left him rather isolated:

[B]eing in a law school is a very uneasy position for me, and I suspect would be for almost any economist. On the one hand, you can’t do the sort of work you would like to do in an economics department. They aren’t interested. You can do it in a law school, but at the same time one really isn’t contributing a great deal to legal education. (Coase in Kitch 1983, 193).

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<sup>66</sup> Telser to the author, June 7, 2018.

<sup>67</sup> See <https://www.coase.org/coasepublications.htm> for references and Medema (1994) for discussion.

Chicago economics was only too eager to claim Coase's Nobel but had little use for his way of doing economics.<sup>68</sup> The feeling was clearly mutual. "Economics as practiced today is the same everywhere you go," Coase said in 2010. "Chicago used to have a distinct program. It still stands at the top of the game, but I don't think it is so much different from other leading places" (Wang 2014, 110). Part of the problem, Coase allowed, was that he was not "aggressive enough to push [his] vision of economics at Chicago" against "the strong presence of Milton Friedman, George Stigler, and other weighty figures" (Wang 2014, 100-101). Had Coase done so, he seemed willing to believe that, in the end, "what [he] called *good economics* might well have prevailed at Chicago" (Wang 2014, 101, emphasis added).

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<sup>68</sup> This comes out most glaringly in Posner's (1993a; 1993b) commentaries on Coase and his work.

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