The *Journal of Comparative Psychology* (*JCP*): A Network Analysis of the Status of Comparative Psychology

Daniel Lahham

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Abstract

Comparative psychology's relationship to various other sub-disciplines and scientific "movements" has been discussed by many scholars throughout its history. The majority of these analyses took the form of frequency counts of the different subject species used within scientific periodicals (Schneirla, 1946; Beach, 1950; Dukes, 1960; Lockard, 1971) and presented similar conclusions: rats were the most commonly researched organism and the study of learning was the key to understanding behavior. The most popular of these critiques was Frank Ambrose Beach's "The Snark was a Boojum" (1950). Beach argued that comparative psychology, with the advent of behaviorism, slowly became a discipline focused only on rat learning in mazes. Donald Dewsbury (1984) responded to these discussions claiming that frequency counts alone could not depict the success and failures of the comparative discipline. Instead, he argued that comparative psychology maintained a historically continuous tradition of excellence off the efforts of a small group of prominent comparative psychologists. In this study, I attempted to "bridge" the gap between these two competing views of the comparative discipline in order to view the legitimacy of both claims. Using network analysis, a tool common to digital history, I investigated metadata (organism studied, scientist, institution) from the *Journal of Comparative Psychology* during the period of 1911 to 1950. I found that both arguments were partially correct in their assertions. Comparative research was being conducted by a small group of prominent scientists throughout the entire four-decade period on many more species other than the rat; however, the broader comparative discipline was heavily impacted by the influx of research on learning in rats. In both cases, the authors inadvertently focused solely on their own claims, and failed to recognize the validity of the other.

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Introduction

On September 7, 1949, Frank A. Beach presided over Division 3 of the American Psychology Association – the Division of Experimental Psychology – and delivered an address as co-president.[1]Beach calculated the proportions of different organisms used as subjects in studies published in the *Journal of Comparative Psychology* and the two journals from which it had descended: *Journal of Animal Behavior* and *Psychobiology*. Beach argued that psychologists were basing their studies disproportionately on the rat in comparison to other organisms in comparative psychology and the entire psychological discipline at large[2]. He argued that for a discipline to be truly comparative, a wide variety of organisms must be used. The rat may indeed still be used but in addition to other mammals, vertebrates, and invertebrates; not the heavy concentration on a single organism that has historically occurred. Since comparative psychology sought to compare organisms, Beach questioned how one could claim to be a comparative psychologist and have one species predominate to such a degree. He further declared that this over-concentration had caused comparative psychology to "suddenly and softly vanish away" (Beach, 1950, p. 115).

Although offered comically and metaphorically, Beach's address (and later article), entitled "The Snark was a Boojum" affected the discipline beyond just the article itself: it set off a series of studies on the different organisms used as test species in the *Journal of Comparative Psychology*. Indeed, every decade since the address, at least one study has been conducted that "took another look at the Snark" in order to determine the rat's rise as a subject species (Dukes,

[1] Beach was originally elected as President by Division 6 – Division of Physiological and Comparative Psychology. After the merger between Division 6 and Division 3 in 1948 -1949, Beach joined W.J. Brogden as co-presidents of Division 3 (Dewsbury, 1996).

[2] The reasons the rat became the standard laboratory are many and will not be discussed here.

1960; Lockard, 1971; Porter, Johnson & Granger, 1981; Grossett & Poling, 1982; Adkins-Regan, 1990; Dewsbury, 1998; Shettleworth, 2009). However, although Beach's concern has received the most attention, he was not the first to draw attention to this rat phenomenon; Theodore Christian Schneirla originally signaled this overuse of the rat in 1946. Indeed, Schneirla and Beach were close colleagues at the American Museum of Natural History during the 1940s (Dewsbury, 1984).

This thesis is not an attempt to "revisit" this Snark phenomenon, nor is it an attempt to explain why the rat (or any other species) came to be such a prolific laboratory animal. This has been done very well already by Cheryl Logan (1999; 2005). Furthermore, this thesis is not a chronological history on comparative psychology. Donald Dewsbury has written an exhaustive history of the discipline and remains one of the authoritative voices on comparative psychology. Instead, in this study, I chose to examine the historical claims made about the successes and failures of comparative psychology as a discipline, and its relationship with various other subdisciplines and scientific "movements." In particular, I attempted to determine how comparative psychology could have disappeared after the 1920s (Beach, 1950) whilst simultaneously prospering in the 1930s (Dewsbury, 1984).[3] To explore these contradictory claims, and reconcile how both could exist simultaneously, I went beyond the assessment of model organisms alone, and included the organisms' experimental relationships with the researchers and institutions that studied them.

Similar to the other "Snark" articles, I extracted my data (organisms, scientists, and institutions) from articles in the *Journal of Comparative Psychology*. Although this journal did not encompass the entirety of comparative psychology's breadth, it was one, significant marker of the field. Since I had access to every article published in the *JCP*, I utilized a "digital"

[3] Both Beach's and Dewsbury's claim were unpacked later in the thesis.

approach for this project. The particular digital approach I employed was that of a network visualization that depicted the connections between organisms and the researchers and institutions that studied them. Through the structures of these networks, I argued that the claims made by Beach in 1950 and Dewsbury in 1984 about the status of comparative psychology as a discipline were both correct at root, but flawed insofar as they both inadvertently failed to recognize the validity of each other's argument.

There was an obvious "data gap" between these two studies, which was likely a contributing factor to their opposite stances. Beach and Dewsbury used only partial runs of journals because they did not have the technology now available to handle large bodies of data. To bridge this gap, I included all the data in the specified time period, and I have expanded its breadth to include not only the organisms studied, but also the scientists and institutions that studied them. This allowed me to expand upon Beach's data – which included only odd numbered volumes, and focused solely on the frequencies of organism use and topics under study. Moreover, by expanding my data to include scientists and institutions in the study, I was able to use these pieces of data to dig deeper into the historical literature, bringing me closer to Dewsbury's extensive qualitative analysis. Thus, my study merged the quantitative (Beach) with the qualitative (Dewsbury).

As was mentioned previously, Beach (1950) contended that comparative psychology disappeared post 1920s, only to be replaced by behaviorism. Schneirla (1946; 1952) similarly argued Beach's point, asking the question, "Have We a Comparative Psychology?" in his critical evaluation of the discipline (Schneirla, 1952).Dewsbury (1984), on the other hand, was much more optimistic in his view of the discipline, claiming that the discipline prospered in a time when most others saw it on the decline. A reason for the discrepancy between these claims

stemmed from how each author assessed the discipline: Beach and Schneirla interpreted the discipline by way of the most studied organisms (rats); and Dewsbury appraised comparative psychology through key events (conferences, publications, etc.) and prolific comparative psychologists (Robert Yerkes, Karl Lashley, Calvin Stone, Harry Harlow, and not least of all Beach and Schneirla).

To demonstrate the accuracy of both claims on the status of comparative psychology, I assessed the influence of two competing, yet similar behavioral approaches: psychobiology and neo-behaviorism. Both approaches indicated the shift in American psychology from the study of internal mental states to the study of overt behaviors at the turn of the century. Moreover, both approaches directly connected to the arguments of Dewsbury, and that of Beach and Schneirla. Dewsbury argued that comparative psychology was maintained and prospered from of the efforts of a handful of prolific comparative psychologists. Since there is no consensus on what, precisely, identifies a comparative psychologist (Dewsbury, 1984), I likened Dewsbury's "comparative psychologists" to the tag of "psychobiologist."Two features bound the terms "comparative psychologist" and "psychobiologist":[4] the study of behavior through the lenses of psychology and/or biology; and the conducting of research outside of the behavioristic framework. Indeed, even Dewsbury himself used the two terms interchangeably on the same individuals (Robert Yerkes and Karl Lashley were both comparative psychologists and psychobiologists) (Dewsbury, 1984; 1991; 2002). Thus, I used the title "comparative psychobiologist" to describe the individuals that Dewsbury claimed maintained the comparative discipline throughout the four decade period. I assessed these comparative psychobiologists at

[4] "Psychobiology" should not be confused with "biopsychology." Whereas "psychobiology" is focused on the biological basis of behavior, "biopsychology" is a branch of psychology that emphasis biological and physiological aspects (Dewsbury, 1991).

the individual and institutional level to investigate whether or not they were indeed as prolific (in terms of quantity and quality) as Dewsbury had argued.

Citing the disproportionate use of rats as subject animals (the model organism of choice in neo-behaviorism),[5] and the rise in popularity of the maze learning studies akin to neo-behaviorism, Beach and Schneirla purported that behavioristic theory essentially replaced comparative psychology post 1920s. As such, I sought to determine the influence of neo-behaviorism in the *JCP* to explore the claim that it "replaced" comparative research. To accomplish this, I examined the most historically prolific neo-behaviorists, and the researchers and institutions most closely connected to the rat (measured in terms of quantity).

In order to determine the influences of both psychobiology and neo-behaviorism, the thesis comprised of three chapters. The first and second chapters covered the period from 1911 to 1950, and focused on the influence of the prominent comparative psychobiologists, indicated by Dewsbury, within the newly established *Journal of Comparative Psychology*. Moreover, the two chapters examined this era diachronically, breaking the time frame into two distinct periods, 1911 to 1930 and 1931 to 1950, in order to elucidate the different generations of comparative psychobiologists. The first chapter began by reviewing the literature on early comparative psychology in order to establish the initial purpose of the discipline of comparative psychology. Next, the first chapter examined the "first generation" of comparative psychobiologists. The second chapter, then, investigated the "new generation" of comparative psychobiologists. The third and final chapter assessed the time frame from 1920 to 1950 in a synchronic fashion, and examined the influence of neo-behaviorism in the *JCP*. Although this time frame was seemingly identical to chapter one and two, each chapter examined different sections within the overall

[5] The term "neo-behaviorism" refers to the reformulations in the 1920s, 30s, and 40s, of Watson's Behaviorism of the 1910s.

network. For example, whereas the networks in chapters one and two placed the emphasis on the number of organisms studied, the networks in chapter two placed the emphasis on number of studies conducted. In chapters one, two, and three, I utilized two different network constructions: the first network displayed the relationship between organisms and authors; and the second network portrayed the relationship between organisms and institutions. Throughout these chapters, I will show that Dewsbury was correct in claiming that comparative psychology (via psychobiology) was maintained by a handful of prominent individuals whilst also demonstrating that through the influence of neo-behaviorism (via the rat) in the *JCP*, Beach's assertion that behaviorism replaced comparative psychology, although extreme in its proclamation, was also correct.

Historiography

Historians have often taken different approaches to writing the history of comparative psychology and the relationships between scientists and organisms. Moreover, each historian has their own distinct interpretation of the historical events they chronicle. For Beach (1950), the history of comparative psychology was a tragedy: comparative psychologists became too enthralled with the rat as a laboratory animal and stopped "comparing" organisms. He appealed to the comparative psychologists of his time, imploring them not to do away with the rat, but to diversify the laboratory organisms they used. Beach went so far as to claim that an overdependence on the rat and learning studies had caused comparative psychology to disappear only to be replaced by behaviorism. In order to make these claims, he collected his data from odd-numbered volumes of the *Journal of Animal Behavior*, *Psychobiology*, and the *Journal of Comparative Psychology*. Beach was interested in which organisms were being studied and in the general topics of the research (e,g., learning, development, reproduction, etc.), of which he

had seven categories. He presented the percentages of articles that used each organism in specified time frames, and the percentage of articles focused on each of his seven topic categories. For Beach, he was not so much concerned with the scientist, as he was with the organisms. Many other scholars and historians have echoed Beach's sentiment and methodology. Porter, Johnson, and Granger (1981) continued Beach's study, and examined the proportions of organisms being used in the *Journal of Comparative and Physiological Psychology* from 1961 to 1976. They reported a similar overdependence on the rat. Beach and Porter et al. both interpreted the history of comparative work through the lens of the most studied organism: the rat.[6]

Beach's 1950 paper was an extension of Schneirla's (1946) study in which he analyzed the articles in the *Journal of Animal Behavior* (*JAB*)from 1911-1917 and the articles in the *JCP* from 1938-1941 (Schneirla, 1946). Schneirla noted some striking findings in the data. The most dramatic finding was the increase in the percentage of articles with rats as subject species that went from 19% in the *JAB* to 66% in the *JCP*. Schneirla also showed that the percentage of studies on invertebrates dropped from 33% in the *JAB* to 5% in the *JCP*. Using this data, Schneirla claimed that comparative psychology was not very comparative at all and had changed with the advent of behaviorism. However, included in his analysis was a caveat that has oft-been overlooked by researchers conducting similar research (Schneirla, 1946; Dewsbury, 1984). Schneirla warned that the *JAB* was a highly interdisciplinary journal, and a large number of the studies were contributed by biologists. He further asserted that the majority of early research on the problems of instincts were conducted by zoologists; psychologists were more commonly studying learning and sensory systems (Schneirla, 1946).

Dewsbury (1984) took a different approach and interpretation of the history of comparative psychology: he chronicled the sub-discipline's history in its entirety, not focusing

[6] Many others have continued this trend: Adkins-Regan, 1990; Dewsbury, 1998; Shettleworth, 2009.

solely on its flagship journal. Moreover, he did not conduct the frequency analyses that Schneirla, Beach, and other "Snark" scholars had done. For him, comparative psychology was not the tragedy that Beach and others had imagined. Instead, Dewsbury saw a new field in a fledging discipline striving to find and sustain success. He interpreted the history of comparative psychology by focusing on the organisms being studied, the important figures that helped build the field, and the major events that took place in the time period he covers. Unlike Beach, Dewsbury emphasized both the scientist and the organism. Moreover, Dewsbury broke down his chapters by each successive decade. His data was not explicitly displayed as it had been in Beach's "The Snark was a Boojum," but was woven into the story he told. He did not write of proportions of certain organisms or percentages of certain types of studies. Instead, he drew attention towards certain landmark events in the discipline. Describing Dewsbury as an optimist would not be an overstatement. The periods in which many viewed comparative psychology as being on the decline, Dewsbury subscribed to an opposing position. For instance, Misiak and Sexton (1966) viewed the 1930s as a period of progressive decline in comparative psychology due to the rise of clinical psychology and the increasing specializations of other psychological fields. Dewsbury, on the other hand, viewed it "as an outstanding decade in comparative psychology" (1984, p. 99). He argued that a new generation of comparative psychologists were produced that were some of the most prominent psychologists of the day. The 1940s had also been seen by earlier authors as a period of decline in comparative psychology (Schneirla, 1946; Scott, 1973). Once again, Dewsbury rejected this, arguing that "the vigor that was generated in the 1930s was sustained in the 1940s" (1984, p. 117), mostly through the continued productivity of the "new generation" of comparative psychologists. Although there were fewer newer faces in the 1940s, the established comparative psychologists were as active as ever. Dewsbury

conducted a masterful synthesis of the key events, institutions, and figures in comparative psychology in the twentieth century, going beyond the *JCP*. Few scholars have gone to the lengths that Dewsbury did in his account of the discipline.

Method

Since this project dealt with a large amount of data (1911-1950), I chose to use digital methods that provide the network visualizations, which, when coupled with traditional methods, provide a powerful new interpretation or understand of the topic. A network is an example of one form of these visualizations.

With initiatives in place to digitize large bodies of text, the number of sources available to historians has greatly increased (Rosenzweig, 2003). These large databases were the impetus for the development of new digital methods. One of the most appealing aspects of digital history is the sheer number of sources that can be included in a single analysis. Computational analysis allows one to analyze hundreds or even thousands of sources that would otherwise be impossible to examine closely even over the course of a single individual's entire career. These computational methods often include computing word frequencies and carrying out various kinds of text mining. The present study benefited from these advantages in two ways: accessibility and size of database. The journal from which the data was drawn has been digitized and is available via the scholarly database PsycInfo. Thus, access to the primary literature was straightforward and efficient. The journals under consideration published over 2000 articles in the relevant time range, from which the metadata was retrieved. In order to analyze this "big data," computational methods were necessary.

Many scholars have been moving towards answering historical questions by using digital tools that mobilize large quantities of data. Indeed, historians of science have shown an interest

in developing new approaches that incorporate both close reading, in its traditional sense, and "distant reading," that is to say, the aggregation and analysis of large amounts of data (Moretti, 2005), which are now made possible with digital historical methods. The exponential growth of physics in post-Cold War America had led David Kaiser (2012) to embrace and advocate change in historical methods. Kaiser argues that this exponential growth has robbed the historian the privilege of solely relying on traditional tools of inquiry such as "close-focus case studies, deep archival excavations, [and] microhistories" (Kaiser, 2012, p. 276). For him, "these tools of inquiry seem to be no match for the brute fact of exponential growth – the extraordinary expansion of people, places, and papers that has marked the scientific at least since World War II" (p. 276). Kaiser's solution was a series of quantitative methods that determined the number of PhDs received and the size and number of journals, making clear patterns in isolated case studies, and making visible questions that can lead to close, archival research.

Extracting large amount of data from open access databases – which I have done in this thesis – is not a new concept. A project conducted by Jane Maienschein and Manfred Laubichler (2010) utilized the open access database, PubMed, as a digital archive providing insight into the history of developmental biology.

As is the case with most methodologies, there are limitations to accompany the many advantages. In the case of digital history methods, the limitations have been well documented by the same scholars that advocate its use. Caroline Winterer (2012) analyzed correspondence networks using Geographic Information System (GIS) mapping – a system to analyze, manage and present geographical data – in order to determine America's place in the Republic of Letters – the long distance community of intellectuals in the 17th century. However, Winterer proposes the caveat that "digitizing humanistic data forces black-and-white answers onto the kinds of

grey-area questions that historians usually delight in tackling" (Winterer, 2012, p. 598). Although this may sometimes be the case, I would argue that the networks continuous spatial layout allows for much finer investigations of the range of greys than one would be able to navigate with mere words. In any case, these limitations outlined by Winterer need not be debilitating as long as we keep in mind that the visualization alone does not provide instantaneous interpretative insights, but rather, is a guiding tool that informs the historian's judgment. The visualizations used in this project were created by using important pieces of metadata coded from the articles and relied heavily on uncovering hidden and salient relationships through the study of the relevant literature (Pettit, Serykh, and Green, forthcoming/2015).

Networks and Data

The data for this project was collected by recording several important pieces of metadata from each substantive[7] article in the *Journal of Comparative Psychology*: year, organism of choice, author(s), and institutional affiliation. The entire run of the journal was available from the scholarly database called PsycInfo. From each article, the year of publication, the type of organism used in the study, the name of the author(s) of the study, and author's institutional affiliation were recorded, by hand, in an Excel spreadsheet. The spreadsheet was then imported into a program called Gephi that produces network images from datasets.

A network graph is made up of two classes of components: nodes and edges. Nodes represent "actors"(which can be nonhuman entities such as animals or institutions). Edges connect two nodes and represent particular kinds of relationships between those particular actors (e.g., node x used node y in research, x published y, etc.). For the present study, the nodes

[7] In this case, a substantive article refers to an original empirical article. Reviews, notes, obituaries, and theoretical articles are excluded.

represented the organism used, and either the first-author of the publication or the institution affiliated with the publication, depending on the particular kind of network. The edges connecting these nodes to represent two distinct relationships: either that between the author and organism, or that between the institution and the organism. Edges can be directed or undirected. Direction conceptualizes the overall network. If a relationship is considered directed, it purports that one node is acting upon another node in some way. For example, a relationship would be directed if one individual sends a letter to another. An undirected network assumes that directionality is unimportant in that particular relationship. Edges can also be weighted; weighted records something quantitative about the relationship between two nodes. For this particular project, the edges were weighted based on the number of publications a particular author had using a particular organism or the number of publications an institution had using a particular organism.

In this thesis, I used a social network software called Gephi. Gephi reads its data from a CSV spreadsheet and can easily export back to a spreadsheet. Not only can Gephi provide aesthetically pleasing network visualizations, but it also provides informative measures for understanding the structure of the network. One of these measures is the idea of centrality within the network. Freeman (1979) classified three forms of centrality: degree, betweenness, and closeness. Degree centrality measures the number of edges that are connected to that particular node. This indicates the overall connectivity of that node within the network, and in my case, makes salient the most prolific authors, institutions, or organisms. A node's degree is directly related to its visibility and importance within the network; the higher the degree, the more visible and important the node. The problem with degree, however, is that an edge that represents a single article by an author about a given organism is indistinguishable from one that represents

five articles by one author about a given organism, unless the difference is represented by something we call "weight" – the thickness of the edge in the network. To resolve this problem, a second degree measure is used that measures the total weight of the links coming into a particular node. This measure, called "weighted degree," was used to determine the most prolific authors, institutions, and organisms in terms of output. Betweenness measures how often a particular node appears on shortest paths between every pair of nodes in the network (Brandes, 2001). It makes visible the nodes that serve as mediators between different parts of the network and is independent of a nodes degree centrality. This measure would be particularly fruitful in determining the most active employee in a workplace. Closeness centrality measures the average distance from a given node to all other nodes in the network (Brandes, 2001). This type of centrality would be most useful in analyzing correspondence networks. Because I am mostly concerned with which actors (nodes) were the most influential within the overall network, only degree centrality was used in my project (Pettit et al., forthcoming/2015). Although degree centrality is the simplest measure, it is often the most useful. Apart from degree centrality, I also used a node's position in the network, often referred to as the spatial layout of the network, to assess the relationship between two nodes. The spatial layout of particular nodes in the network is highly dependent on both degree and weighted degree. How far a node is positioned from the center of the network depends on how many distinct connections it makes. The more connections a node makes, the more integral they are to the overall network structure. Moreover, weighted degree influences the force or strength a particular node has over nearby nodes.

Two separate networks were created; one that covered the data from 1911 to 1950; and another that covered the period of 1920 to 1950. Moreover, the networks were constructed in two

different ways: an author based network that represents an author-organism dyad, and an institution based network representing an institution-organism dyad.

Source (*JCP*)

The data was taken from the *Journal of Comparative Psychology* (*JCP*) from the period of 1911 to 1950. The data consisted, specifically, of organisms, authors, and institutions. This is the current title of the journal (since 1983), but it has operated under three other titles – *Journal of Animal Behavior* (*JAB*), *Psychobiology* (*PSB*), and *Journal of Comparative and Physiological Psychology* (*JCPP*) (Burkhardt, Jr., 1987; [Cover], 1947). The journal originally began as the *JAB* and ran from 1911 to 1916. After the journal could not financially sustain itself, it was dropped and effectively replaced by *PSB* in 1917. *PSB* would not last long, printing only two volumes in 1917/18 and 1920, before changing names to the *JCP*. The *JCP* was established in 1921 as the combination of the interests of the *JAB* and the *PSB*. The *JCP* operated under the editorship of Knight Dunlap until 1943, when one of his colleagues, Roy M. Dorcus, took over editorial duties. Dorcus held editorship until 1947, when Calvin Perry Stone became editor and the journal's name changed to the *JCPP*. In 1983, the journal split into *Behavioral Neuroscience* and the reinstated *JCP*.

Journal of Animal Behavior

The establishment of this journal owed much to the diligent work of Robert Yerkes. Indeed, the *JAB* is often considered to be Yerkes' journal (Burkhardt, Jr., 1987). At the turn of the century, the study of animal behavior was gaining momentum. Yerkes, who established the animal psychology laboratory at Harvard and arranged a book series focused on animal behavior with the Macmillan Company, grew impatient with the lack of a journal specifically dedicated to publish animal work. He sought the advice of the zoologist, Herbert Spencer Jennings, about

establishing such a journal in 1903, a year after he graduated with his doctorate. Jennings suggested that the Carnegie Institute of Washington might fund the endeavor, but it appeared that Carnegie had other intentions with their money (Burkhardt, Jr., 1987). Yerkes was forced to move in a different direction, becoming an editor for the *Journal of Comparative Neurology* (*JCN*) in 1904. The *JCN* was founded by C.L. Herrick in 1891 as "a quarterly periodical devoted to the comparative study of the nervous system (Herrick, 1891). Yerkes joined the likes of Herrick, his brother C.J. Herrick, and O.S. Strong, and was put in charge of all work published by the journal in comparative psychology and animal behavior; topics that the journal newly adopted. This change in the journal's scope was indicated by the change of its title to the *Journal of Comparative Neurology and Psychology* (*JCNP*) in 1904.[8] Unfortunately, most journals were not self-supporting enterprises at this time and the new *JCNP* was in financial peril. Yerkes had made many attempts to solve the issue, but faced too many impediments (Burkhardt, Jr., 1987). The journal needed more subscriptions but faced heavy competition from the *Biological Bulletin* and the newly established *Journal of Experimental Zoology* (*JEZ*) in 1904. By 1906, Jennings and many other collaborators of the *JCNP* had felt that animal behavior work was better suited for the *JEZ*. Yet, the *JEZ* suffered the same financial difficulties as the *JCNP*. Both would be purchased by the Wistar Institute of Anatomy and Biology in Philadelphia in 1907 along with the *Journal of Morphology*, the *American Journal of Anatomy*, and the *Anatomical Record*. Jennings and Yerkes saw this centralization as a chance to better concentrate materials on animal behavior; unfortunately, this hope was not realized, and both men began looking for another way of concentrating animal research. Although Jennings was full of doubt that an animal behavior journal could self-support, Yerkes pursued the matter in earnest in the following years (Burkhardt, 1987).

[8] For a more detailed account of the reasons and justifications for this change, see Burkhardt, Jr., 1987.

By January, 1910, plans were in place for a new journal, and Yerkes and Watson had begun the process of selecting an editorial board. Although many names were suggested, the editorial board ultimately consisted of prominent zoologists S.J. Holmes (Wisconsin), Jennings (Johns Hopkins), and W.M. Wheeler (Harvard), and significant psychologists M. Bentley (Illinois), H.A. Carr (Chicago), E.L. Thorndike (Columbia), Watson (Johns Hopkins), M.F. Washburn (Vassar), and Yerkes (Harvard), who served as managing editor ([Cover], 1911). The *JCNP* announced the new *Journal of Animal Behavior* late in 1910 (Herrick, 1910). Although the editors of the *JCNP* expressed regrets that they were losing Yerkes and his two associates, Watson and Jennings, they wished the new journal success, and recommended that any papers regarding comparative psychology or animal behavior be sent to the *JAB*. In 1911, eight years after Yerkes had first expressed to Jennings the need for a journal for work on animal behavior, the *JAB* published its first issue. The *JAB* would soon face financial issues of its own, which led to its discontinuation in 1917. Yerkes had managed to keep the journal afloat through 1915 based on contributions from the editors themselves, but with escalating printing costs in 1916, the journal ran a deficit. Yerkes discussed the idea of having the Wistar Institute take over financial responsibility for the journal, but the initiative was fruitless. Even raising the subscription price from $3.00 to $5.00 was not enough to save the journal. Financial concerns were not the only reason the journal did not survive. The *JAB* rested on the efforts of Yerkes, and in 1916 and 1917, Yerkes took on many other initiatives that occupied up much of his time. He served as president of the APA in 1916/1917; he organized the Army intelligence testing program during the war; and he chaired the Psychology Committee of the National Research Council (Burkhardt, Jr., 1987).

Psychobiology

In May 1917, Knight Dunlap approached Yerkes with the suggestion that the *Journal of Animal Behavior* be merged with his soon to be established periodical, *Psychobiology* (*PSB*). Yerkes immediately rejected the offer, claiming that it would not be advantageous to combine the interests of the two journals (Burkhardt, Jr., 1987). Nonetheless, *PSB* published its first issue in July 1917. It ran two volumes, one in 1917/1918, and the other in 1920. In his opening editorial, Dunlap said that the journal's primary aim was "the publication of research bearing on the interconnection of mental and physiological functions" which included work on "physiological psychology…pharmacology, physiology, anatomy, neurology and psychiatry in so far as the results of these investigation have explicit bearing on problems of mental life, or mental factors are included in the essential conditions of the investigation" (Dunlap, 1917, p. 1). In addition to Dunlap (Johns Hopkins), the journal's editors were J.J. Abel (Johns Hopkins), W.B. Cannon (Harvard), R. Dodge (Wesleyan), S.I. Franz (Government Hospital), H.S. Jennings (Johns Hopkins), and G.H. Parker (Harvard).

Journal of Comparative Psychology

In 1921, the *JCP* was established, replacing both *JAB* and *PSB* (Burkhardt, Jr., 1987). Yerkes and Dunlap served as co-editors for this journal; although Dunlap served as managing editor and took primary control of the journal. Up till this point, Yerkes was still heavily involved with his chairmanship of the Research Information Service of the National Research Council. The remainder of the editorial board consisted of the amalgamation of the editors of the *JAB* and *PSB*. The *JCP* would continue publishing until its name change in 1947.

Journal of Comparative and Physiological Psychology

In 1947, the *JCP* was renamed the *Journal of Comparative and Physiological Psychology* ([Cover], 1947). Calvin P. Stone served as the new managing editor for this journal, joined by an

impressive list of consulting editors: F.A. Beach (Yale), C.W. Darrow (Chicago), R.M. Dorcus (California), H.F. Harlow (Wisconsin), W.T. Heron (Minnesota), H. Klüver (Chicago), K.S. Lashley (Yerkes Laboratories), C.T. Morgan (Johns Hopkins), E.C. Tolman (California), and G.R. Wendt (Rochester) ([Cover], 1947). The journal would run under this name until it split into *Behavioral Neuroscience* and the *Journal of Comparative Psychology* (for a second time) in 1983. It is not well documented why the journal's name was changed to include physiology in its title. According to historian Don Dewsbury, "the field was changing and the physiological types, growing rapidly, felt that the name no longer was appropriate for their work" (Dewsbury, 2012, email correspondence).

Chapter One: First Generation of Psychobiology in the *Journal of Comparative Psychology*

Determining a date that marked the beginning of comparative psychological work is a difficult task and a debate on its own. Dewsbury (1984) chose to begin his history with a prehistory dating back nearly 34 000 years. Eventually, he designated the year 1894, not as the beginning of comparative psychology, but as his preferred starting point. Although Dewsbury considers 1894 to be somewhat of an arbitrary choice, his decision was guided by important intellectual events of the year, some of which were: the death of von Helmholtz and Romanes; Margaret Floy Washburn took her PhD at Cornell under Titchener; John Dewey moved to the University of Chicago and Münsterburg was at Harvard; James McKeen Cattell and James Mark Baldwin established the *Psychological Review* as an alternative outlet to the house organ *The American Journal of Psychology*; and the year 1894 saw the publication of C. Lloyd Morgan's *An Introduction to Comparative Psychology*. Following Dewsbury, I chose 1894 as the most appropriate starting point for this project.

1894-1899

As Dewsbury aptly suggested, the year 1894 was important for the emergence of comparative psychology. American Psychology was preparing for the explosive growth that the turn of the century would bring: twenty-nine psychology laboratories had been established across the United States by 1894 (Garvey, 1929). In the same year, G. Stanley Hall, a staunch advocate for the grounding of scientific psychology in the model of Darwinian evolutionary theory, was serving in his sixth year as President of Clark University. Baldwin, another proponent of evolutionary theory in psychology, was at Princeton where the year prior he established his second psychological laboratory (the first was at Toronto in 1890). Baldwin later reopened the Johns Hopkins laboratory in 1903. One of Baldwin's most important contributions to the study

of evolution, and by extension comparative psychology, was his proposal of "organic selection" in 1896. This process was later dubbed the "Baldwin effect" (Simpson, 1953). Essentially, the "Baldwin effect" explained the process by which a nonhereditary (acquired) trait might come to be hereditary without resorting to Lamarckian mechanisms. Also in 1894, Baldwin teamed up with James McKeen Cattell to found the *Psychological Review*, a journal to rival the *The American Journal of Psychology*. Baldwin and Cattell originally proposed one of two options to Hall: buying *The American Journal of Psychology* from him, or at least establishing a board of editors with powers to accept and reject submissions. Hall refused both options even though he admittedly had grown tired of the duties of an editor (Sokal, 1997). The *Psychological Review* held importance for comparative psychology, since much early comparative work was published in this journal. A journal dedicated solely to comparative work, however, was not established until the *Journal of Animal Behavior* (*JAB*) in 1911.

The year 1894 also saw significant events in Europe that helped shape comparative psychology. It was in this year that Wilhelm Wundt's *Lectures on Human and Animal Psychology* was translated by J.E.E. Creighton and E.B. Titchener and first published in English. Wundt described two different approaches to animal psychology: a comparative approach where animals (including humans) were studied for the purpose of understanding the development of the organic mind, and an alternative approach in which animals were studied solely for their utility in understanding the human mind. Not only did Wundt's *Lectures* have a profound influence on the legitimacy of animal psychology and the method of objective behavioral observation, but his recognition of a purely comparative approach with no reference to its human relevance was an important step in establishing the field of comparative psychology (Dewsbury, 1984). This prompted Warden's remark that "even so conservative a writer as

Wundt…recognized the right of comparative psychology to develop a content independent of human reference in the same sense as zoology is independent of human morphology and physiology" (Warden, 1928, p. 514).

In England, C. Lloyd Morgan's publication of *An Introduction to Comparative Psychology* has been referred to by Dewsbury as "the most memorable event" of 1894 (Dewsbury, 1984, p. 45). The book outlined trial-and-error learning, "constraints" on learning, and accepted the idea that perception should be viewed from the organism's perspective. Morgan's book rejected the view of Larmackian[9] inheritance of acquired traits (Dewsbury, 1979; 1984). A staunch supporter of the study of instinctual behaviors, Morgan wanted to separate inherited behavior from learned behavior. For him, every species has its own instinctive behaviors that do not have to be learned; the discussion of "consciousness" in animals can only be had if the behavior cannot be explained by any other way. Indeed, Morgan's canon states:

> In no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale (Morgan, 1894, p. 53).

Morgan would figure prominently in the coming post-World War I debates on instinct.

A few years later, at Clark University, Linus Kline and Willard S. Small conducted the first psychological tests of rats in mazes, an event that set the standard for animal psychology and comparative psychology in the twentieth century (Dewsbury, 1984). Kline first came to Clark University in 1896 to study "zoological psychology." Interested in what he termed *home sickness*, Kline studied homing in bees, pigeons, and chicks. Dewsbury describes Kline's use of rats and mazes as a somewhat serendipitous event. Kline was working with chicks and reading

[9] Lamarckian inheritance or Lamarckism is the process of an organism passing on a trait to its offspring that was acquired, not inherited. It was named after the French biologist and evolutionary theorist Jean-Baptiste Lamarck (1744-1829) (Sapp, 2003; Gould, 2002).

Morgan's *Introduction* when the idea of trapping rats in small boxes to study their food searching behavior occurred to him. The inspiration for the use of rats came from a Clark colleague, biologist Colin C. Stewart, who studied the effects of alcohol, diet, and barometric pressure on rat behavior. By the fall of 1898, Kline had devised a study in which rats learned how to get food from "mouse boxes." On December 3, 1898, Kline undertook his first study of learning in rats (Dewsbury, 1984). A little over a month later, on January 9, 1899, Kline conducted a study on social interactions, in which two rats had to interact to secure food from a box. The use of a maze, however, was the idea of Edmund C. Sanford, professor of psychology and founding director of the Clark psychology laboratory (Sokal, 1990). Kline described to Sanford "runways [he] observed several years ago made by large feral rats to their nests under the porch of an old cabin" (cited in Miles, 1930, p. 331). Sanford promptly suggested to Kline the use of the Hampton Court maze. Kline subsequently modified the design out of an encyclopedia, and the first maze was constructed by Small. Small noted in his study on rat maze learning (Small, 1901) that the natural propensity of the rat to navigate winding passages was a critical reason for their decision to study rats using mazes. In terms of the first use of white rats, Stewart began his studies using wild, "gray" rats before moving onto tame white rats. On his switch, he wrote, "If anyone wants to know why I changed from wild gray rats to white rats in 1895, let him work with gray rats for a year" (cited in Miles, 1930, p. 334). Stewart got his white rats from C.F. Hodge, a biologist specializing in neurology and physiology at Clark (Baldwin, 1901). Around the same time, another neurologist and student of Hall's, Henry H. Donaldson, began a colony of white rats at the University of Chicago. Donaldson eventually established the Wistar Institute of Anatomy and Physiology in Philadelphia that became famous for their Wistar strain white rats. The Wistar Institute would come to supply many of the most active psychology

laboratories in the United States. Further information on Donaldson's and the white rat's influence on psychology is provided by Logan (1999; 2001; 2005).

The individuals who would later impact comparative psychology were just beginning their studies in 1894: Robert Mearns Yerkes was an undergraduate at Ursinus; Edward Lee Thorndike was an undergraduate at Wesleyan; and John Broadus Watson was a freshman at Furman University.

Edward L. Thorndike moved from Wesleyan to Harvard in 1896 to begin his graduate studies. Working with young chicks in the cellar of William James' home,[10] Thorndike's Harvard research on the instinctive nature of pecking behavior in chicks was published in 1899 (Thorndike, 1899a). Thorndike did not stay long at Harvard, leaving in 1897 to complete his PhD at Columbia. Dewsbury cited personal and financial reasons as the cause for the move. In 1898, at Columbia, Thorndike completed his thesis entitled, *Animal Intelligence: An Experimental Study of the Associative Processes in Animals*, which immediately became a classic study in the history of animal learning. The text primarily dealt with learning in dogs, cats, and chickens. Thorndike famously used his puzzle boxes that required an animal to interact with a lever or other device in order to escape the box and secure food. Thorndike found that the animals solved problems in a trial and error fashion, and did not display reason. His thesis was so influential that the results were summarized and published in *Science* (1898) and in the article "Do Animals Reason?" in *Popular Science Monthly* (1899b). Thorndike also conducted one of the first ever studies of learning in fish, demonstrating escape learning in killifish (*Fundulus*) (Dewsbury, 1984).

[10] Thorndike's landlord would not permit the chicks in his room and James was unable to secure laboratory space. Thus, Thorndike resorted to conducting his studies in the cellar of William James' home.

Evidently, this six-year period was extremely important in the development of a comparative psychological discipline. The future giants of the field were nearing the start of their influential careers: Yerkes entered graduate school at Harvard and in 1899 transferred from zoology to psychology; and Watson was one year removed from his historic move to the University of Chicago. In 1899, Sanford at Clark, and G.H. Mead at Chicago, began offering courses in comparative psychology. Moreover, the first two comparative psychology laboratories were placed by Warden and Warner (1927) at Clark, under Sanford, and Harvard, under Yerkes.

1900-1909

The twentieth century began much the same way as the nineteenth century ended, with a large degree of optimism towards comparative psychology. As C. Judson Herrick proclaimed in 1907, "comparative psychology [had] arrived" (Herrick, 1907, p. 76). Further to the optimism that surrounded comparative psychology, this was a time when psychology moved towards experimental design over naturalistic observations, and when anecdotal evidence was replaced by data derived from experiments (Yerkes, 1943; Dewsbury, 1984). It was also a time when learning theory started to gain serious traction in comparative psychology. Still, the comparative psychologists at the time understood the importance of natural observation and of studying different aspects of behavior (Dewsbury, 1984).

Some of the first learning studies at the turn of the century belonged to Small (1900; 1901) and Thorndike (1901). Using Kline's puzzle boxes, Small placed white rat's on the outside and food in the inside of the box. This was opposite to what Thorndike had done previously, placing the animal in the box and the food outside. Small, like Kline, was extremely vigilant about keeping animals in the most naturalistic environment as possible. This was due in large part to the orientation of Small compared to Thorndike; Small believed animals do things "only

in line of its inherent abilities" (Small, 1900, p. 133), and thus, drew inspiration from Morgan, whereas Thorndike cited Lubbock and preferred a laboratory method approach (Dewsbury, 1984). In Small's study, the rat had to dig into the box to get the food, and the digging behavior was considered in line with the natural characteristic of the rat. In Thorndike's study, having the animal trip a lever or latch to attain food might not have been as naturalistic a setting, but gave the researcher the desired experimental control. As learning theory became more dominant in the coming decades, Thorndike's approach became the precedent, and only a few remained loyal to the more naturalistic approach that Small advocated. In 1899, Thorndike began researching his fourth species after receiving three cebus monkeys and housing them in his residence on West 123rd Street in New York (Joncich, 1968; Bitterman, 1969). Similar to the conclusions derived from his thesis, Thorndike found that the monkeys did not show any signs of reasoning or imitation behavior. Furthermore, although Thorndike did admit that the monkeys could learn to solve problems other mammals could not, he attributed this to a difference of degree, not one of kind. This conclusion that learning did not differ substantially across a wide diversity of species had serious implications for the future of comparative learning studies, and animal psychology in general (see Beach, 1950; Logan, 2001).

During the first decade of the twentieth century, three institutions led the way on comparative studies on learning: Harvard, Clark, and Chicago. The Harvard animal lab was headed by Yerkes, a Harvard pupil who had just graduated with his doctorate in 1902. Yerkes, who figures prominently in the next chapter, studied learning in a wide range of species, including turtles (1901); green crabs (1902); frogs (1903); crawfish (Yerkes and Huggins, 1903); and dancing mice (1907). His master's student, Lawrence W. Cole (1907) added raccoons to the list of species being studied by those affiliated with Harvard (Pettit, 2010). Although much of

Yerkes' early work was on learning, he was interested in a whole range of behavioral patterns. He would become renowned for his work in the field of primatology and his championing of the chimpanzee as a model organism.

The Clark laboratory was headed by Sanford and, although much of the early work was done by Small and Kline, other Clark workers were conducting comparative experiments as well. A.J. Kinnaman (1902), for instance, conducted a laboratory experiment on rhesus macaques. The monkeys were expected to interact with and manipulate a number of devices. Unlike Thorndike, who claimed there was no evidence of reasoning or imitation in his studies with monkeys, Kinnaman found that the monkeys did exhibit reason and imitation. According to Dewsbury (1984), Kinnaman was arguably the first to study monkeys in a laboratory setting. However, credit for the first use of monkeys belongs to Thorndike. Another Clark student, James P. Porter, conducted comparative learning studies on spiders (1906a), and on English sparrows, vesper sparrows, and cowbirds (1904, 1906b). Porter also studied "instinctual" behaviors such as mating, web building, and feeding in spiders. One of Sanford's doctoral students, Herbert Burnham Davis, studied intelligence in raccoons, along with a variety of instinctive patterns. Alongside Cole and Walter S. Hunter, Davis was one of three major figures in the history of raccoon research (Pettit, 2010). Cole and Davis both began studying learning in captive raccoons in 1905, and although conducted separate experiments, both studies had much in common. Both utilized the newly developed puzzle boxes on a novel organism, with the intention of assessing the raccoons' "comparative place on the scale of animal intelligence" (Pettit, 2010, p. 399).

Meanwhile, John B. Watson received his doctorate in 1903 at Chicago, and remained there as an instructor. The majority of Watson's work was done primarily on white rats: his well-received dissertation (Watson, 1903) was a study on the behavioral development of white rats.

This study initiated Watson's meteoric rise in psychology. However, there were those who were critical of Watson's method of sacrificing rats for brain studies. Watson's work began with the caring of Donaldson's colony. He was not, however, limited to rats only. In 1906, he solicited the university to purchase for him four monkeys. The monkeys were used for a study of imitation (1908). Like Kinnaman, Watson believed that imitation existed, and he expressed the importance of further studies on the phenomenon. In the years 1905 and 1906, the careers of Watson and Yerkes intersected via a correspondence, before diverging sharply in later years (Watson became a radical behaviorist while Yerkes called himself a psychobiologist). The importance of this interaction is highlighted by the status of both men as future giants in the field of comparative psychology. Furthermore, Watson's early allegiance with Yerkes was contrasted by the shaky relationship between the Harvard and the Clark labs. Indeed, Yerkes believed that although comparative psychology was alive and well at Clark, it was "indiscreet or bad form for a Harvard psychologist to try to cultivate friendly professional relationships with Hall and his Clark associates" (Yerkes, 1943, p. 75). This was due in part to the long-standing rivalry between William James and Hall, as well as differences in the publishing habits of the two institutions: Harvard workers tended to publish brief papers focused on learning in animals; Clark workers published longer articles that included extensive introductions, life histories, and points on methodology (Dewsbury, 1984). And although Watson did not engage in many comparative studies, his student, C.S. Yoakum (1909), conducted learning studies on squirrels and compared them to other species.

By the end of the 1890s, comparative psychology laboratories had been established at Harvard and Clark and comparative courses offered at Chicago and Clark. By 1910, comparative courses were offered at Cornell, Drake, Harvard, Ohio State, Iowa, Johns Hopkins, Michigan,

Mount Holyoke, Oklahoma, and Texas. Moreover, comparative psychology laboratories were established at Chicago, Cornell, Johns Hopkins, Michigan, and Texas (Warden &Warner, 1927). Evidently, comparative psychology was growing rapidly in America during this decade. Watson proclaimed that "comparative psychology has completely justified its existence" (1906, p. 155). He was wary, however, that many topics had been breached and not enough refined. Coupled with this lack of refinement was the inadequacy of the available facilities. Thus, with the support of Baldwin, Watson called for an experimental station devoted to the study of animal behavior (Dewsbury, 1984):

> The need to the psychologist of an experimental station for the study of the evolution of the mind is as great as is the need to the biologist of an experimental station for the study of the evolution of the body and its functions (Watson, 1906, p. 156).

1911-1950

At the turn of the century, the discipline of psychology was changing. Psychologists were concerned with their scientific status, leading to a divide in regards to methodology, subject matter, and goals of the discipline (Burnham, 1968; O'Donnell, 1979, 1985; Samelson, 1977). Not only was psychology in the process of emancipating itself from philosophy, but it was also assessing its distance from biology, and determining the degree to which its utility was able to justify its existence. Under the pressure of these circumstances, Robert Yerkes and John Watson, among others, defined and developed an experimental science based on the study of animal behavior, culminating in the establishment of the *Journal of Animal Behavior* in 1911; one of the first institutionalizations of the study of animal behavior (Burkhardt, Jr., 1987). However, although Yerkes and Watson were both advocates for the experimental study of overt animal behavior, they both took similar, but different approaches. In 1913, Watson published his

"Psychology as the Behaviorist Views It" in the *Psychological Bulletin*. This important publication essentially became the behaviorist manifesto. Watson called for psychologists to leave behind the attempt to study subjective, conscious mental states by means of introspection, replacing it with the objective experimental study of overt behaviors (Cohen, 1979; Dewsbury, 1984; Benjamin, 2007).Watson envisioned the development of a complete program that would emphasize the study of behavior in nonhuman organisms and eventually extend its principles to human behavior.[11] Behaviorism or late-Watsonian behaviorism soon thereafter came to resemble an extreme reductionist[12] approach, eliminating any influence of inherited factors. Even though Yerkes similarly sought to distance himself from psychology as the study of mental experience, he did not subscribe to the behavioristic framework purported by Watson and others. Instead, Yerkes moved towards psychobiology "as an integrative discipline with physiology" (Dewsbury, 1991, p. 200), and established a program of comparative psychobiology at Harvard (Yerkes, 1930). This research orientation allowed Yerkes to study overt animal behavior without being shackled to the increasingly popular behaviorism. Yerkes considered himself a psychobiologist rather than a psychologist. Indeed, his autobiography was titled "Robert Mearns Yerkes: Psychobiologist" (Yerkes, 1930).

Yerkes was not the first individual to use the term psychobiology. In fact, Dewsbury (1991) has traced the earliest use of the word to Willard S. Small (1901) in his paper that began research on maze learning. Knight Dunlap also considered himself a psychobiologist in the same way that Yerkes did. However, the term psychobiology has had many diverse meanings throughout history. Yerkes and Dunlap distinguished between three different clusters of uses for

[11] The fourth chapter will discuss behaviorism in more detail.

[12]Reductionism is a philosophical position that holds that a phenomenon is nothing more than the sum of their parts, and the object of study could be understood by its individual constituents (Jones, 2000; 2013)

psychobiology: the first one was the way it was used by E.S. Russell; the second cluster was Adolf Meyer's use of the term; and the third their own meaning of the term.

Edward Stewart Russell was a prominent British biologist whose *Form and Function* (1916) sought to change classic morphology into a functional morphology. For Russell, the organism – that he perceived as purposive – had to be studied holistically, including both the morphological with behavior. He termed this approach "hormic biology" (Russell, 1923; 1924). His use of psychobiology came in his opposition to vitalism. Russell argued that it was purposiveness that distinguished between living and non-living things and that purpose was studied through a psychobiological (functional) method in biology.

The Swiss-born psychiatrist, Adolf Meyer was a long time professor of psychiatry and director of the Phipps Clinic at Johns Hopkins University. He used psychobiology to define his holistic approach to psychiatry as the integration of social, psychological, and biological factors to better treat patients (Meyer, 1915). Psychobiology was a way for Meyer to deal with "the functions of the whole person and not merely as detachable parts" (Meyer, 1915, p. 861). Dewsbury (1991) also noted that most definitions of psychobiology define it in the way Meyer used it. This is not surprising considering the influence Meyer had on Dunlap, Watson, and many others in this era (Dewsbury, 1991; Billings, 1939).

In 1914, Knight Dunlap published a textbook entitled, *An Outline of Psychobiology*. The book most closely resembled what would today be called physiological psychology (Dewsbury, 1991). Dunlap believed that "[he] raised this term from its grave, and employed it as covering the consideration of biological materials and theories from the point of view of the bearings of these on the problems of psychobiology" (cited in Dewsbury, 1991, p. 199). Dunlap also founded and served as executive editor of *Psychobiology*, a periodical that published two volumes in 1917

and 1920. Psychobiology, for Dunlap, was the interconnection between mental and physiological functions, and could also be called "physiological psychology." Moreover, Dunlap's psychobiology also encompassed investigations in pharmacology, physiology, neurology, anatomy, and psychiatry as long as these investigations had bearing for problems in mental life (Dunlap, 1917). Although Yerkes accepted Dunlap's use of psychobiology, his research interests were much broader and his work was less physiological and more behavioral. Yerkes also applied the term to distance himself from mainstream psychology, in particular, behaviorism.

It appeared that Yerkes and Dunlap at least believed they were using the term similarly (Yerkes, 1934). However, there appears to be little continuity in the way they used the term compared to Meyer's use. Dunlap, in particular, believed that Meyer used the term in a philosophical manner, which he claimed bore no resemblance to his usage of the term. Meyer echoed these differences in how he used psychobiology compared to Dunlap and Yerkes. He believed Dunlap's use was too narrow and academic, and indicated the difference between him and Yerkes was the latter's acceptance of mental causality (Dewsbury, 1991). For the purposes of this study, I used Dunlap and Yerkes's integrated definition of psychobiology. That is to say, "psychobiology" as an integration between psychology and physiology, and as a way to distinguish behavioral research from behaviorism.

Before beginning the analysis of Dewsbury's claims on the status of comparative psychology in the twentieth century, it is important to unpack some of his arguments. He was not convinced by the type of frequency counts employed by Schneirla, Beach, and other writers who revisited the "Snark." For Dewsbury, the percentage of comparative research studies on the lab rat did not matter nearly as much as the quality of comparative research and its impact on the wider discipline. He believed that focusing on quantity and percentages alone actually served to

distort the image on the history of comparative psychology. His main thesis in the book however, was that although comparative psychologists were a small part of the wider psychological discipline, their achievements were noteworthy. Indeed, he argued that these same comparative psychologists developed and maintained the comparative discipline, and provided reason to be proud of the history of the discipline rather than apologetic. He asserted that a community of comparative psychobiologists maintained "a consistent thread" of excellence throughout the history of the comparative discipline.

Although Dewsbury acknowledged the achievements of many prominent comparative psychologists, he focused mostly on a handful of prominent psychobiologists that were highly impactful in maintaining comparative psychology. According to Dewsbury, during the 1910s, three individuals were key contributors to the development and establishment of comparative psychology, Yerkes, Watson, and the newly arrived Karl Spencer Lashley. However, although early a proponent of the study of animal behavior and comparative psychology, and highly influential in the development of the comparative field, Watson moved away from comparative psychology in the mid 1910s towards human based research.

Dewsbury recognized the early 1920s as a transition period for comparative psychology. Lashley had just taken up a position at the University of Minnesota where one of his students and future significant comparative psychobiologist, Calvin Perry Stone, graduated. But mostly, the early 1920s was a time ripe with controversy over instincts and behaviorism, with many (neo) variations on behaviorism being offered. However, Dewsbury claimed that the return of Yerkes to academia in 1924, coupled with the influence of Lashley and the graduation of Stone, H.C. Bingham, and Carl J. Warden provided the boost necessary for comparative psychology to prosper in the late 1920s and early 1930s.

It was Dewsbury's characterization of the 1930s that was most distinct from other historians. Historians like Misiak and Sexton (1966) described the 1930s as a time of decline in comparative psychology. Moreover, Beach claimed that the 1930s saw the greatest rise in the percentage of research articles on the rat. However, Dewsbury argued that the graduation of a "new generation" of prominent comparative psychobiologists made this decade one of the most notable in the history of comparative psychology. Including Yerkes, Lashley, and Stone, Dewsbury acknowledged the importance of the arrival of Norman Raymond Frederick Maier, Harry Friedrich Harlow, Winthrop Niles Kellogg, and Frank Ambrose Beach in the 1930s. These comparative psychobiologists were also largely responsible for Dewsbury's characterization of the 1940s as a decade of sustained vigor.

Dewsbury's argument is predicated on the achievements of these prominent comparative psychobiologists. It is for this reason, then, that I have chosen to track these individuals throughout the period of 1911 to 1950 in the *JCP*. Although Dewsbury has accurately depicted the qualitative prominence of these individuals' achievements, I argue that these comparative psychobiologists were also quantitatively prominent. I measured their quantitative prominence by assessing how many organisms they published on in the *JCP*, and the total number of publications they contributed to this journal, both at the individual and institutional level. Thus, I conclude that Dewsbury was correct in his assessment of comparative psychology as a small, yet vigorous discipline. I decided to focus on two generations of comparative psychobiologists over two chapters. The first generation, covered in chapter one, included Yerkes, Lashley, and Knight Dunlap. The second generation, covered in chapter two, was made up of Stone, Maier, Harlow, Kellogg, and Beach. Dewsbury did not consider Dunlap a comparative psychologist because he studied mostly humans. However, I argue that his close connection to the *JCP* as executive

editor, his self-identification as a psychobiologist (Dewsbury, 1991), and his research on four different organisms not only qualified him as a comparative psychobiologist, but made Dunlap one of the most influential individuals in comparative psychology's history. My argument will be constructed as follows: I begin with a biographical sketch of the three "first" generation comparative psychobiologists and then assess their individual and institutional influence on comparative psychology via the information derived from the networks. The higher their influence in the network, the more confident I am in arguing for Dewsbury's characterization of comparative psychology as a small, important discipline maintained by a handful of prominent psychobiologists.

Network Visualizations

In this section, I introduce visual networks that depict the metadata collected from the *Journal of Comparative Psychology* (*JCP*) from its inception in 1911 to the year 1950. The metadata collected from the *JCP* was obtained strictly from substantive articles (reviews, notes, and theoretical papers were excluded) and included: year, organism studied, first author,[13] and institutional affiliation. The data was then organized in a spreadsheet and imported in the software Gephi. Two different forms of networks were created, one depicting the relationship between organism and author, and the other depicting the relationship between organism and the institution. For this project, I constructed my argument based on three aspects of the network.

The first aspect was assessing the authors and institutions connected to the most diverse array of organisms. This is measured using degree. A high degree is indicative of many distinct connections, and a low degree is indicative of very few distinct connections. In this project, a high degree represents the use of a comparative research approach or the use of a wide diversity

[13] Including any co-authors distorts the quantitative measures in the network. If a co-author was prominent enough, they would eventually appear as a first author.

of organisms. The second aspect was the total number of connections (not necessarily distinct) between an author/institution and the organisms they studied. This is measured using weighted degree, which takes into account all instances of a relationship between an author/institution and the organisms. The higher the weighted degree, the more articles were published by an author or institution. Thus, a high weighted degree indicates the most prolific authors/institutions in terms of output. The third and final aspect was the determination of a particular author's position in the network. This is often referred to as the spatial layout of the network. I expect that the most comparative authors, connected to the most organisms, would be positioned nearest to the center of the network. In contrast, the authors connected to only one organism would be positioned more peripherally in the network. I argue that authors positioned near the center of the network were more integral to the overall structure of the network, and most likely to have been mediators or conduits between different organism clusters.

Reading the networks from the vantage point of these three aspects allowed me to infer certain things about the comparative psychobiologists under study. For instance, a high degree indicated a researcher had studied multiple organisms in a particular time frame, and thus, had been comparative in their research program. In the context of my argument, studying multiple organisms was an important marker of a comparative psychobiologist. Weighted degree indicated which researchers were the most prolific or productive contributors (quantity) to this journal in a specified time frame. These productive researchers, at least quantitatively, were some of the most active in the field, and thus, were influential in the maintenance of the comparative discipline. Degree and weighted degree are not only useful measures for assessing the specified researchers at the individual level, but they also contribute to the structural positioning of a particular researcher in the overall network. Degree dictates how far a researcher

will be positioned from the center of the network and how many different communities they connect, and weighted degree influences the force or strength a particular researcher has over nearby nodes. For example, if a researcher node has a higher weighted degree than an organism node, the researcher node will have a stronger gravitational pull. In this study, I expected the specified comparative psychobiologists to have been highly comparative (high degree) as well as having been prolific contributors to the journal (high weighted degree). With these high centrality measures, I also expected them to be positioned as mediators, connected to multiple communities near the center of the network, with stronger than average gravitational pull.

The First Generation: Robert Mearns Yerkes, Knight Dunlap, and Karl Spencer Lashley

Robert Yerkes was arguably the most important individual in the establishment and maintenance of comparative psychology. Not only did he establish the *Journal of Animal Behavior*, but his efforts helped develop an experimental program of comparative psychobiology in an era dominated by behavioristic theories. Yerkes was first educated at Ursinus College, receiving his A.B. degree in 1897. In the same year, he moved to Harvard where he took his A.B. degree in 1898, and his A.M. degree in 1899. Interest in zoology and psychology led Yerkes to comparative psychology and, at the suggestion of Josiah Royce, Yerkes transferred over to the psychology department to work in animal psychology. He received his PhD in 1902 under Hugo Münsterberg (Yerkes, 1930) and subsequently founded the Harvard laboratory of comparative psychology (Dewsbury, 1984). Yerkes remained at Harvard in various roles -- assistant, instructor, and professor -- for the next fifteen years. As the Great War broke out, Yerkes accepted a position at the University of Minnesota as chair of the psychology department, which was newly separated from the philosophy department, but he never physically went there. Yerkes was integral in mobilizing psychologists during World War I, and remained as an administrator

after the war had ended. In 1924, he moved to Yale as Professor of Psychobiology, where he remained until his retirement in 1942.

Like Yerkes, Knight Dunlap was also a self-proclaimed psychobiologist who was extremely influential to comparative psychology and the *JCP* (Dewsbury, 1991). His education began at the University of California where he received his BPhil in 1899. From there, he moved to Harvard where he took his PhD in 1903. He encountered many of the same mentors and influences as Yerkes did (Münsterberg, Royce, Dewey, and James) since both were at Harvard during the same period. In 1904, he took a faculty position at Berkeley before moving to Johns Hopkins University in 1906. He remained there for three decades, leaving in 1936 to establish a psychology laboratory and graduate program at the University of California, Los Angeles (Dorcus, 1950; Moore, 1949; Dewsbury, 1984). Dunlap was also vehemently against the current definitions of instincts;[14] the only one of the three psychobiologists with this disposition. Coupled with his doctrine that good research resulted from assuming the opposite stance, Dunlap was an unpopular iconoclast (Dorcus, 1950).

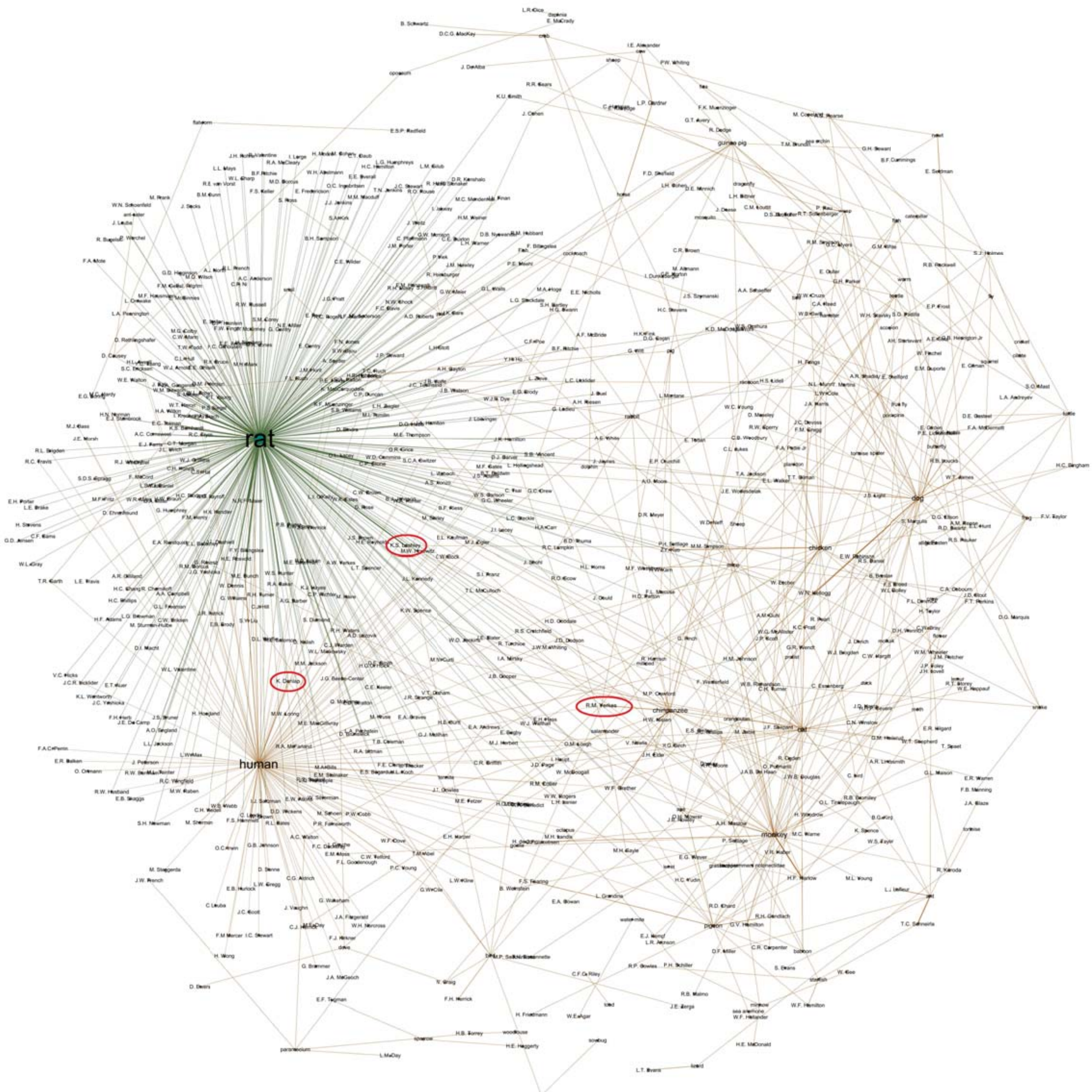
The third comparative psychobiologist, Karl Spencer Lashley, was different from the other two in that he never self-identified as a psychobiologist. Rather, the label was applied to him by the historian, Donald Dewsbury (2002). Dewsbury's justification was, first, that his initial training was a PhD in zoology that he obtained under Herbert Spencer Jennings at Johns Hopkins University in 1914. Thus, much of Lashley's work was biologically oriented. And second, through a series of learning experiments in the 1920s, Lashley dismantled the stimulus response theories of the day. Clearly, he was interested in behavior but not sympathetic to the behaviorist approach. He was, however, influenced by Watson during his graduate training,

[14] The concept of "instincts" have been an important and controversial debate throughout the entire history of the psychological discipline (see Dewsbury, 1984 for a detailed account).

conducting field studies with Watson on terns on the Dry Tortugas in 1913. After attaining his doctorate, he worked with Shepherd Ivory Franz before joining the Minnesota faculty in 1917. He remained there until he went to the Institute for Juvenile Research (affiliated with the University of Illinois in Chicago) in 1926. His next stop was the University of Chicago in 1929. He left for Harvard in 1935, remaining there for the remainder of his career until 1955. While at Harvard, from 1942 to 1955, he held the position of director of the Yerkes Laboratories of Primate Biology.

Psychobiologists in the Network

The first network I looked at included the entire period of 1911 to 1950 (Figure 1). The network was weighted by using the number of organisms studied (degree). In other words, emphasis was placed on the number of organisms one studied rather than the number of studies published. Before determining how these three psychobiologists were depicted in the network, it is important to describe what one is seeing. This network consists of 760 nodes (authors and organisms) and 894 edges. Often, an author might have been studying multiple organisms, indicating why there are more edges than nodes in this network. Of the 760 nodes, 57 were organisms and 703 were authors. From a "bird's eye" perspective, the network appeared as a collection of clusters of various sizes. The largest cluster was made up of 348 distinct authors studying the rat for a total of 747 published articles on the organism (degree=348, weighted degree=747). The next largest cluster consisted of 127 distinct authors studying the human and publishing a total of 185 articles (d=127, wd=185). This trend of the rat and human being the two most studied organisms was seen throughout all versions of the networks. Table 1 presents the fifteen largest organism clusters in terms of degree and weighted degree, and Table 2 shows the top fifteen most comparative and productive researchers.


rat
human
K.S. Lashley
K. Dunlap
R.M. Yerkes

The three comparative psychobiologists, Yerkes, Lashley, and Dunlap, boasted not only a comparative approach, but were some of the most highly productive researchers in the *JCP* from 1911 to 1950. In terms of studying multiple organisms, Yerkes had the most comparative approach of any other author in the *JCP*, studying eight different organisms: rat, human, chimpanzee, monkey, dove, worm, pig, and bird. Lashley studied five different organisms: rat, bird, cat, monkey, and chicken. And Dunlap studied four different organisms: rat, human, bird, and rabbit. All three psychobiologists studied both birds and rats, Yerkes and Lashley examined monkeys, and Dunlap and Yerkes ran experiments on humans. (Yerkes' experiments on humans came during his time in the war effort).

Despite the commonalities in species choice, each of the three had their own preferred organism: Yerkes favored the chimpanzee; Lashley preferred the rat for his neurological and perception research; and Dunlap felt that studying the psychological conditions of humans were most fruitful. Some might argue that Dunlap's preference of the human made him a non-comparative psychobiologist (see Dewsbury, 1984), but I argue that his studies on the rat, bird, and rabbit displayed his comparative breadth. Yerkes and Lashley both began by studying a number of different organisms before "finding" and sticking with their respective favored model. Dunlap took a different approach, beginning his studies on the human, moving to the rat, rabbit, and bird, before eventually returning to the human. Yerkes believed that anthropoids were the closest organism to humans, and thus, chose the chimpanzee as the ideal subject to investigate sexual and social behavior for the "welfare of mankind" (cited in Biehn, 2008, p. 21). Lashley's research revolved around neurological bases of learning, anatomical structures of the brain, sensation and perception, and the effects of drugs on learning (Hebb, 1959). The small, abundant, and inexpensive rat was the most amenable and logical organism for this type of

research. Dunlap's interest in the field of vestibular research – which he picked up from a brief stint in the army – led to his and his students' choice of human subjects (Dorcus, 1950).

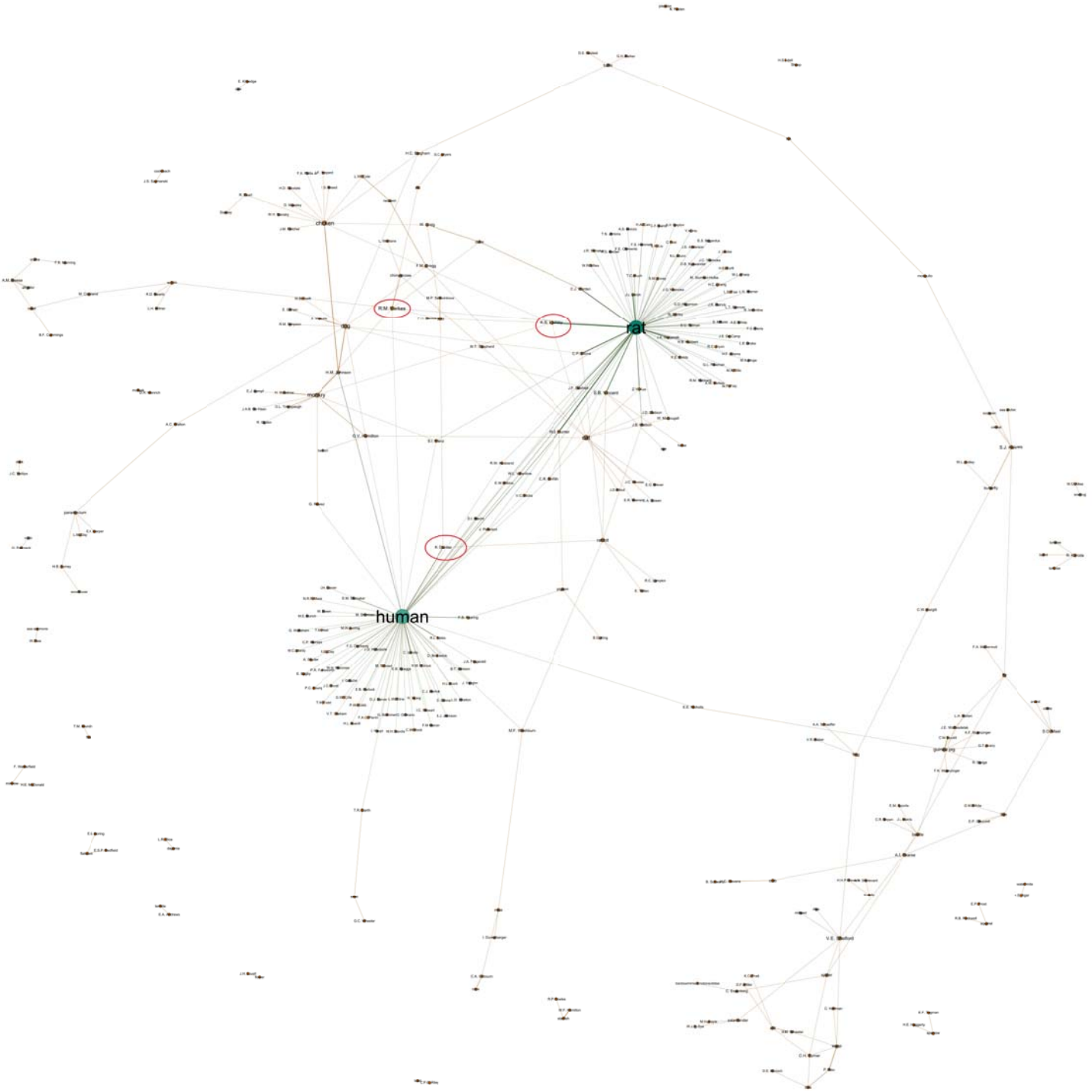
In terms of the quantity of research published in the *JCP*, these three psychobiologists also were among the top. From 1911-1950 Karl Lashley published 14 articles, Robert Yerkes contributed 12 papers, and Knight Dunlap authored 11. The majority of each researcher's repertoire, however, was focused on their preferred standard organism, establishing a close connection between the two. Along with degree, this close connection contributed to their positioning as close to the node representing their desired organism. For instance, Yerkes' strong connection to the chimpanzee positioned the two very nodes close to one another in the network. Moreover, since Yerkes employed a highly comparative approach, their representative nodes were located closer to the center of the network than the periphery. Similarly, Lashley's close connection with the rat kept him close to that species than any of the other four he studied. His connection to five different organisms also kept him close to the center of the network. Finally, Dunlap's affinity to the human brought him close to that species cluster, and his degree of four kept him nearer the center. As I mentioned earlier, I argue that the closer a node (in this case a researcher) is to the center of the network, the more influential to the overall structure of the network and of the discipline itself. When the network is weighted on degree, in order to be near the center, a node must have multiple connections throughout the network. Thus, scientists near the center of the network are connected to multiple organism clusters, acting as conduits that presumably mediate the flow of information between communities of researchers and organisms. If we look at the degree and weighted degrees of these psychobiologists, and by consequence, their position in the network, we can argue that not only were these researchers some of the most comparative and productive in the discipline, but they were also mediating information between

different "communities" of researchers and organisms. In this respect, Yerkes, Lashley, and Dunlap were integral in maintaining the comparative discipline. However, such a large time period (1911-1950) does not allow us to make any claims on smaller time intervals.

The Early Years (1911-1930)

The period from 1911 to 1950 provided a general view of the discipline over several generations. However, it was difficult to discern any information about the early years when viewing the data over such a long time frame. For this reason, I split the time period of 1911-1950 in half, focusing on the early years from 1911 to 1930, and the later years from 1931-1950. The network covering the period of 1911 to 1930 (Figure 2) consisted of 313 nodes and 335 edges. This new, smaller network, comprised of two dominant clusters, the human (d=71, wd=100) and the rat (d=70, wd=119), a number of smaller clusters (cat, monkey, chicken, dog), and a few dyadic and triadic clusters (see Table 3 for full degrees and weighted degrees of fifteen largest organism clusters). Similar to Figure 1, this network was weighted by degree, and thus, a researcher studying diverse organisms would once again be positioned nearer to the center of the network.

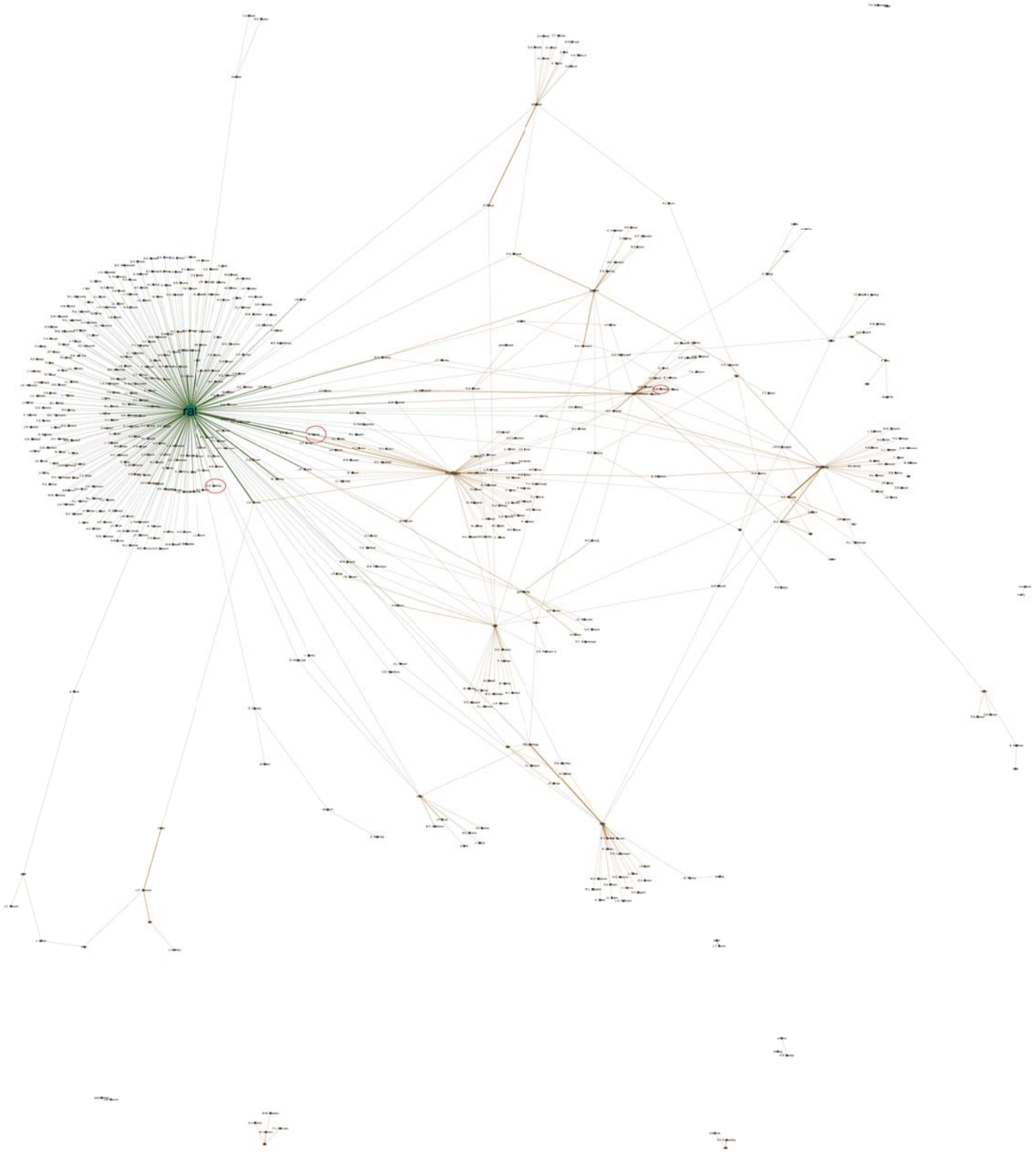
In Figure 2, Yerkes, Lashley, and Dunlap were circled to make them easier to spot. The majority of Yerkes's work came within the years of 1911 to 1930 (only three articles were published by Yerkes in the *JCP* after 1930). Even in this smaller network, Yerkes (d=8, wd=9) had the highest degree of any author, and thus, he was positioned near the center of the network. His high occurrence with the chimpanzee kept him closest to that organism, rather than the other organisms he studied, like the human or the rat. The opposite occurred as well: whereas the chimpanzee, monkey, pig, bird, dove, and worm influenced where Yerkes was positioned, he also influenced where these organisms were positioned in the network.


rat
human
mouse
monkey
dog
chicken

Indeed, Yerkes had a higher degree and weighted degree than the chimpanzee, worm, pig, bird, and dove. In simpler terms, Yerkes, an author, was connected to more species than the number of distinct researchers that were studying either one of the previously mentioned organisms. A node with a higher degree has a higher gravitational force in the network, drawing closer nodes with smaller degrees. Similar to Yerkes, the majority of Lashley's publication output came in the period of 1911 to 1930. With a degree of five and weighted degree of 12, Lashley published twelve articles in this period, with only two articles on the rat published in the *JCP* after 1930. The comparative Lashley was also located near the center of the network, however, his nine articles on the rat kept him closer to that organism. Lashley was also connected to the relatively large cat, monkey, and chicken clusters. Dunlap, in opposition to the other two, was equally active before and after 1930, in the *JCP*. From 1911 to 1930, Dunlap studied three organisms for a total of six articles. Of the three authors, he was the only one who did not study the rat in this network. Instead, he conducted four studies on the human, and one each on the bird and rabbit. His studies on three organisms placed him in the center of the network, but closer to the human cluster. In this period, all three psychobiologists were positioned in the network to have presumably been mediating the flow of information between multiple organism communities.

The Later Years (1931-1950)

Figure 3 depicts the next time period, from 1931 to 1950. This network consisted of 526 nodes and 591 edges. The rat cluster exponentially eclipsed the human cluster in this time frame. The rat (d=293, wd=628) was studied nearly eight times more than the next closest organism, the human (d=61, wd=85). (Table 4 depicted the degrees and weighted degrees of the top fifteen organism clusters). The rise of the rat could be indicative of the spread of neo-behavioristic theory in the 1930s – even though the rise of the rat as a subject species was a discipline wide


ral

phenomenon (see Logan, 2002, 2005) – or could simply be an artifact of more data in this network than the other. The increase in the number of articles published in this period caused each cluster to grow as well, but none reached the rat's growth rate: in the period after 1930, four times as many researchers studied the rat, and five times as many articles were published on the rat (all figures taken from the *JCP*) compared to the next closest organism.

Since the majority of Yerkes's and Lashley's work was done prior to 1931, it was no surprise to find that they each had a degree of one and a combined weighted degree of 5 (Yerkes published three articles and Lashley published two). Indeed, Yerkes retired as director of the Yale Laboratories of Primate Biology (YLPB) in 1942, and began writing his *magnum opus*, *Chimpanzees: A Laboratory Colony* (Biehn, 2008).[15] He also resumed his position in the war effort during World War II (Dewsbury, 1984). Lashley succeeded Yerkes as the new director of the Yale Laboratories of Primate Biology in 1942 and conducted much important primate work himself (Carmichael, 1959). However, none of this work on primates was published in the *JCP*. In fact, Lashley stopped publishing in the journal after he moved to Harvard in 1935. Even though Lashley and Yerkes did not publish in the *JCP* in these later years, their respective "standard" organisms, the rat and chimpanzee, continued to be studied more often. Knight Dunlap was fairly active, studying the human and the rat during this time period, and producing a total of five articles. Although he was not one of the more prolific contributors in this network, Dunlap's connection to the rat and human positioned him as one of the few authors who mediated the two largest clusters in the network.

Viewing the network in three different time periods, I was able to determine the extent and timing of these psychobiologists' influence in comparative psychology. Robert Yerkes, the founder of the *Journal of Animal Behavior* and the initial driving force of a comparative

[15] For a full account of Yerkes's retirement, see Biehn, 2008.

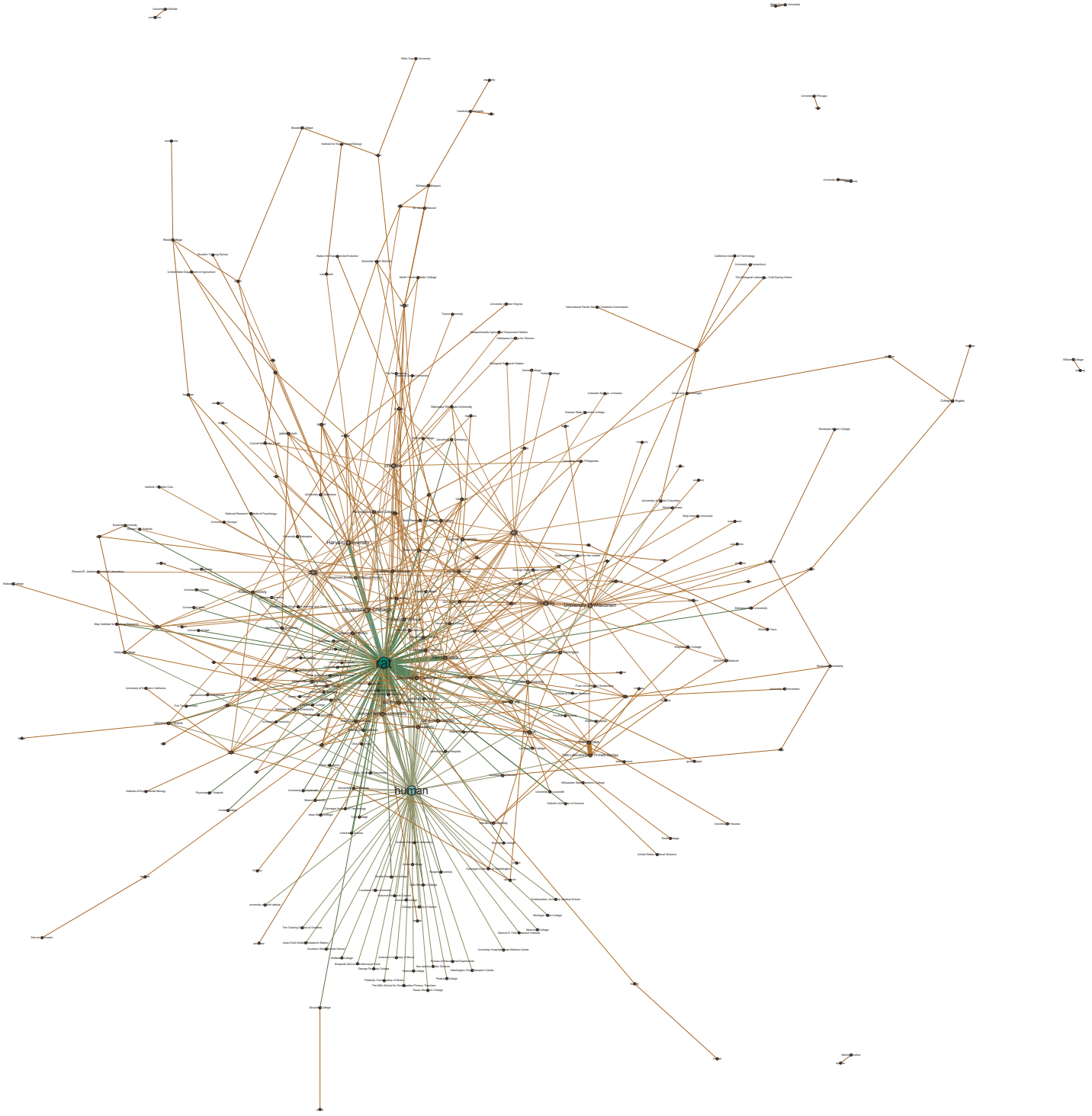
psychobiology, studied the most diverse array of organisms of any other researcher. In terms of production, Yerkes conducted twelve studies, which was modest amongst the leaders. By breaking the time period in half, it became apparent that the vast majority of Yerkes' work came prior to 1930, explaining his modestly high output. Karl Lashley exhibited a similar trend to Yerkes insofar as most of his research output came in the period before 1930. Lashley contributed a total of fourteen articles on five organisms. Dunlap was less comparative and prolific overall than Yerkes and Lashley, publishing eleven articles on four organisms. However, unlike Yerkes and Lashley, Dunlap was equally as active before and after 1930. With this information in hand, it appeared Dewsbury was astute in characterizing Yerkes, Lashley, and to a lesser extent, Dunlap, as the driving force for the development and establishment of a comparative discipline in the 1910s and its maintenance in the transitional period of the 1920s. Indeed, after the 1930s, Dewsbury placed the success of comparative psychology in the hands of the newly graduated generation of psychobiologists.

Up till now, we have only looked at one type of network depicting the relationship between researcher and organism. Thus, we have been able to determine the influence of these comparative psychobiologists on an individual basis. In order to determine their influence at the institutional level in developing and maintaining the discipline, a different type of network needed to be used depicting the association between organisms and institutions. The next section tracked trends in organism choice at the institutions these psychobiologists were affiliated with. In particular, I sought to determine how much of an institution's experimental approach to the study of animals (comparative or a single model organism) was attributed to the psychobiologist they employed and whether or not the prominence of that institution was associated with the prominence of their resident psychobiologist. I argue that if the institutions that employed

Yerkes, Lashley, and Dunlap were prominently contributing to the *JCP* in a similar manner to their in-house comparative psychobiologist (prominence measured by quantity of publications and the diversity of organisms studied), then the psychobiologists along with their institutions were influential in sustaining the comparative discipline.

Institutional Affiliation of the Psychobiologists

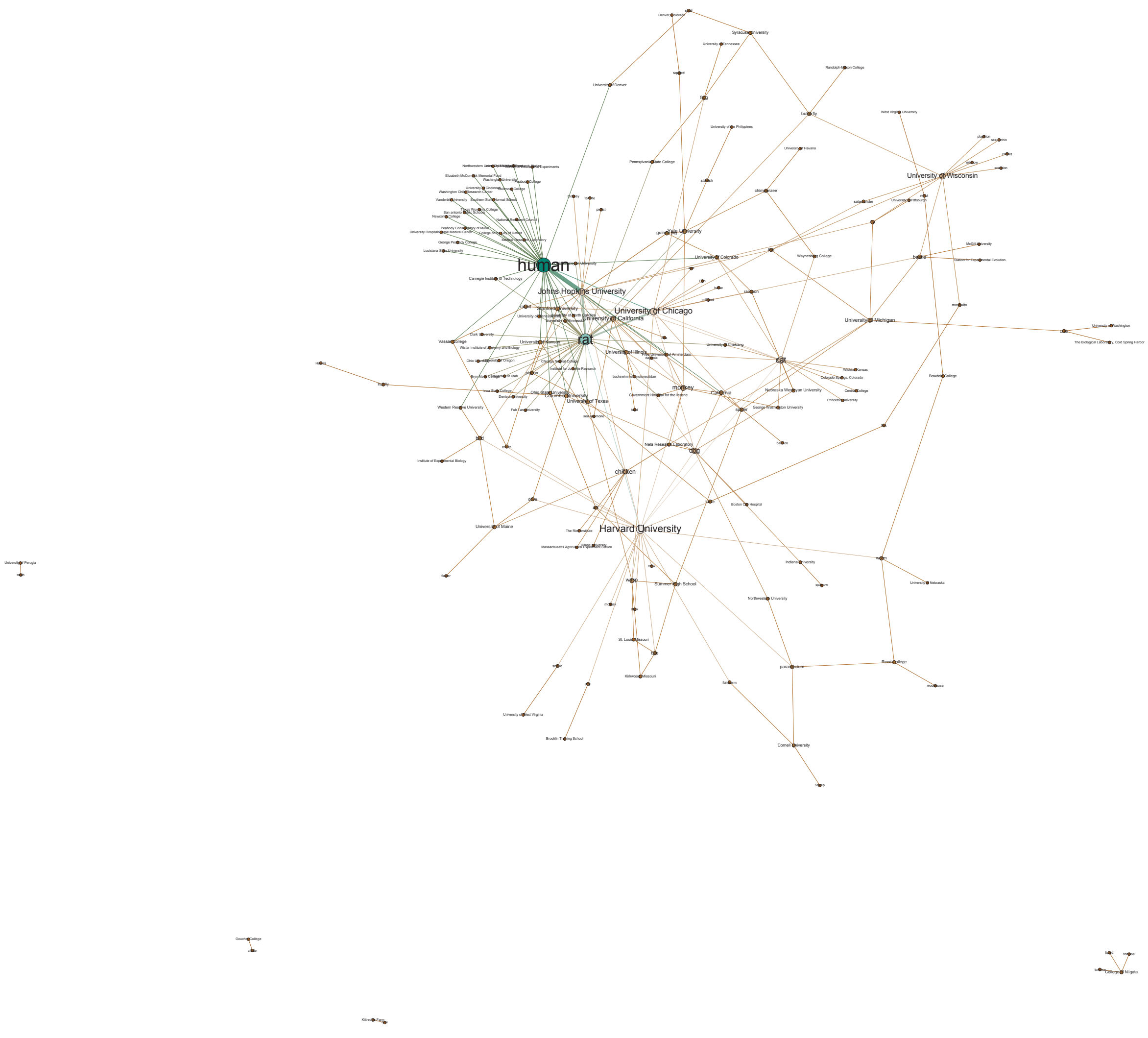
There was a large amount of overlap between the institutions at which these three psychobiologists researched and taught, and thus, their careers were interconnected. For instance, at the same time Yerkes took his doctorate at Harvard, Dunlap began his graduate training at the same institution. The connections did not stop there, however. Dunlap held a position as professor of psychology at Johns Hopkins when Lashley arrived as a student. When Lashley graduated from Hopkins, in 1917, he was hired by Yerkes as an instructor at Minnesota. Although Yerkes did not physically go to Minnesota, he still made hiring decisions. Indeed, 1917 was a busy year for Yerkes, who assumed the presidency of the APA, and chair of the Committee on the Psychological Examination of Recruits where he oversaw the administration of the Army Alpha and Beta tests (Yerkes, 1930; Benjamin, 2007). Yerkes moved to Yale in 1924 where he would spend the rest of his career; the latter portion of which was spent as director of the YLPB. Lashley went from Minnesota to the Institute for Juvenile Research in 1926, before taking a position at the University of Chicago in 1929. Later, Lashley went to Harvard in 1935 – making all three psychobiologists affiliated with Harvard at some point in their careers – and stayed until his retirement in 1955 (Hebb, 1959). He also succeeded Yerkes as the director of the YLPB. Dunlap was at Johns Hopkins until 1936 when he moved to UCLA to establish a graduate program in psychology (Zusne, 1975).


rat
human
Harvard University
University of Wisconsin

In Figure 4, the relationship between institution and organism is depicted for the period of 1911 to 1950. The network consists of 278 nodes (225 were institutions and 53 were organisms) and 512 edges. Table 5 shows the top six institutions by degree and weighted degree. Since I was interested in the institutions that these psychobiologists were stationed at, and because the time in which they were there varied in the period from 1911 to 1950, I was unable to determine any association between the researcher and his institutional influence in this large network. Thus, I viewed the data in the arbitrarily chosen time frames used earlier: 1911 to 1930, and 1931 to 1950. This allowed me to examine Harvard when Yerkes was there, John Hopkins under Dunlap, and so on.

Institutions in the Early Years (1911-1930)

Figure 5 depicts the institution-organism network from 1911 to 1930. This network consisted of 178 nodes and 247 edges. During this period, Yerkes was at Harvard and Yale, Dunlap was at Johns Hopkins, and Lashley was at Johns Hopkins and Minnesota. Thus, for this time period, I was only interested in tracking the trends in these four institutions. Yerkes, studying the most diverse array of organisms in the *JCP*, established the comparative psychology laboratory at Harvard and, thus, it is fitting that Harvard researchers boasted the most comparative laboratory of any institution. To be sure, Yerkes was executive editor of the *JCP* (then titled *Journal of Animal Behavior*) from 1911 to 1916, and determined how his home institution was to be represented while he was there. Of the institution's 22 different subject species, 20 of them were studied in this period. Furthermore, the Harvard laboratory had not yet decided on a "standard" with which to work: only nine of 29 articles were published on an already studied organism. Indeed, Yerkes had not found the chimpanzee until his return to academia in 1924, when he had long since left Harvard. Rosalia Abreu, prior to the war, had


human
rat
Johns Hopkins University
University of Chicago
University of California
Harvard University
University of Wisconsin
University of Michigan
chicken
dog
monkey
cat

contacted Yerkes about visiting her vast collection of primates in Havana. Abreu had successfully managed the conception and birth of a chimpanzee, the first person to do so in the Western Hemisphere. Her collection included marmosets, monkeys, baboons, mandrills, apes, gibbons, orangutans, and chimpanzees. He began his investigations on apes in the summer of 1924 (Dewsbury, 1984).

Yale University was not particularly productive in this period. Of the 70 articles published by Yale's comparative psychology researchers, only 16 appeared in this period across six different species. Much of this inactivity was due to the dismantling of the Yale psychology department at the turn of the century. Edward W. Scripture, a former doctoral student of Wilhelm Wundt's, founded the Yale psychological laboratory in 1892 (Benjamin, 2007; Garvey, 1929); although some credit the founding to George Trumbull Ladd[16] (Burnham, 1971). With the arrival of a new President, Arthur Twining Hadley, and the subsequent philosophical differences, Scripture was forced to resign, Ladd was dismissed, and the psychology department folded until James Rowland Angell succeeded Hadley as President of Yale (Kuklick, 2004; Burnham, 1971). Angell brought in Yerkes in 1924, and immediately Yerkes began his psychobiological work with chimpanzees under the Yale patronage (specifically: Yale Laboratories of Comparative Psychobiology). Yerkes and Angell revived the Yale psychology department until Clark L. Hull arrived in 1929 and turned it into a hub for neo-behavioristic research (Benjamin, 2007; Sears, 1982; Rumbaugh, 1997).

During this entire period, Knight Dunlap was stationed at Johns Hopkins. Hopkins researchers, with a degree of 14, were the third most comparative laboratory (behind Harvard

[16]Ladd was a psychologist in his writings and philosopher by title, but during a time when most psychological programs were still part of philosophy departments, this was not an uncommon occurrence. He was brought to Yale in 1881 to teach philosophy and psychology and soon thereafter headed up Yale's department of Mental Philosophy (Burnham, 1971).

and Chicago). However, Hopkins was the most productive laboratory at the time, publishing nearly double the number of articles as their closest competitor: Johns Hopkins researchers published 62 studies compared to the 32 by Chicago researchers. Twenty-nine of these studies were on the human, the "standard" organism in Dunlap's psychobiological research. Much of Dewsbury's research was focused on child, industrial, and abnormal psychology. The rat was also commonly studied in this period, albeit by only a few researchers: of the 15 rat studies, four were by Lashley and five by the graduate student J.L. Ulrich. Moreover, rabbits, which Dunlap studied in 1925, featured in four articles.[17] It appeared that Dunlap had greatly influenced the "standard" organism of his laboratory, but also influenced the output of his department. By 1921, Yerkes and Dunlap co-founded the *Journal of Comparative Psychology,* and although both men were executive editors, Dunlap took on the majority of the editorial duties (Burkhardt, Jr., 1987; Moore, 1949). I would argue that Dunlap exerted his influence as executive editor and favored the research coming from his own institution in addition to studies on humans. *The American Journal of Psychology* has been similarly criticized as G. Stanley Hall favored the research of friends and students, both former and present (O'Donnell, 1985). I believe the *JCP* served a similar purpose for Dunlap, although further research is needed.

Lashley contributed eight of his 14 articles while at Johns Hopkins. Four of these articles were on the rat, and the other four indicated the extent of his comparative approach (monkey, chicken, cat, bird). Lashley was at Minnesota in various capacities from 1917 to 1926. Yerkes hired Lashley in 1917 as instructor before resigning his post to take up his position on the National Research Council. Yerkes replaced himself with his Harvard assistant and colleague in the Army Testing Project, Richard M. Elliot (Department History, 2013; Hebb, 1959). Upon his arrival, Lashley was surprised to find no Yerkes and a deficient working environment. Although

[17] This might have been due to the convenience of already having a rabbit in captivity.

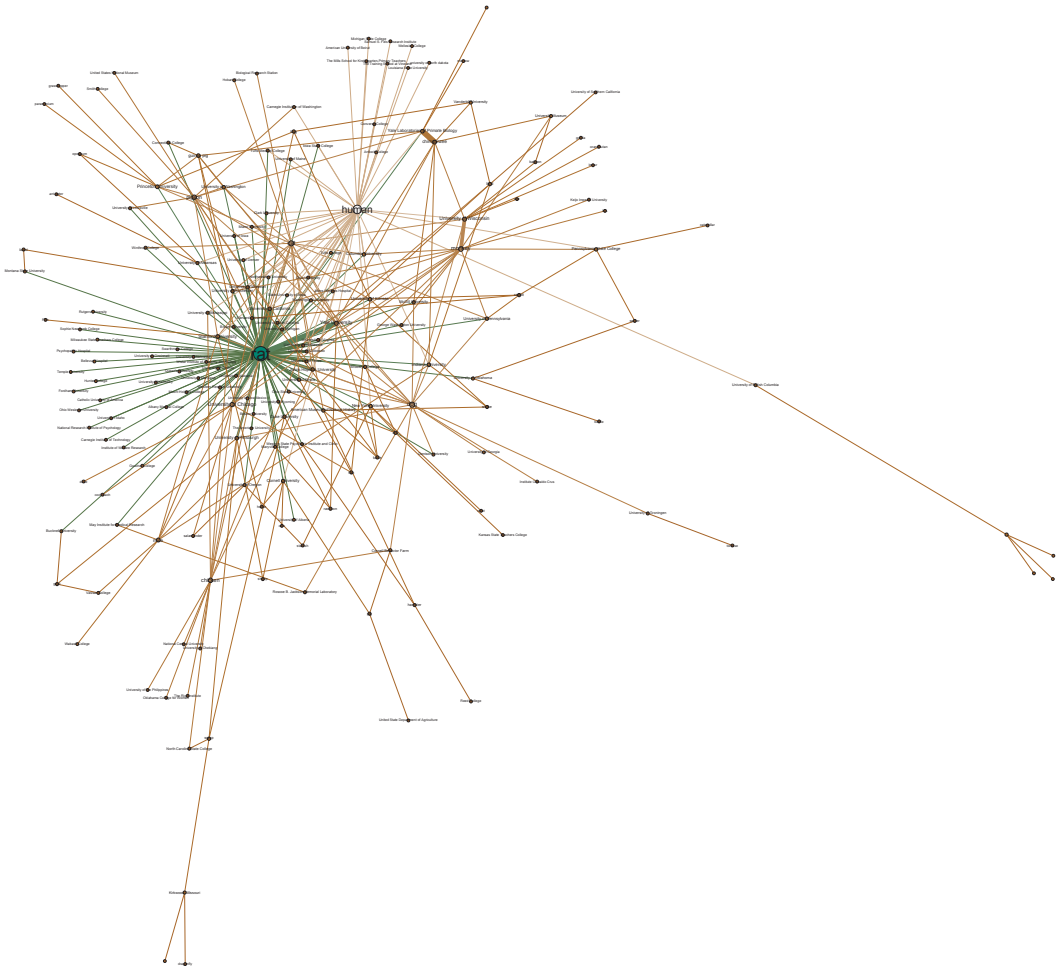
Lashley taught at Minnesota in 1917, he did not publish under its affiliation until Elliot had appointed him assistant professor in 1920, relieving him of his teaching duties. Lashley was never particularly good at teaching, and it was a research position he was after (Hebb, 1959). Like Yale, Minnesota was not particularly productive in this period with only 15 articles on two species (rat, human). However, the Minnesota Philosophy and Psychology department did not split into two separate departments until 1917 (Department History, 2013), and thus, the psychology laboratory was just a fledging enterprise. Still, Lashley's influence early on was strong. Quite literally, Lashley or his students were the only individuals publishing on any species under the Minnesota affiliation in the *JCP* from 1920 to 1929. This included Carney Landis' important dissertation on human emotional behavior that appeared across four papers (1924; 1925; 1926; 1926), Calvin P. Stone's (1922) dissertation on the copulatory activity in the male rat, and the articles of two of his female students, Ruth M. Hubbard (1927) and Mary Shirley (1928; 1928; 1928), who published their dissertations on discrimination in learning and neural activity of the rat.[18] Lashley published three articles on the rat while at Minnesota. He left in 1926 to the Institute for Juvenile Research where he published one article on the rat. The next section focused on the years from 1931 to 1950.

Institutions in the Later Years (1931-1950)

Figure 6 depicts the time period of 1931 to 1950 and consisted of 189 and 322 edges. In this time frame, Yerkes had his Orange Park facility in Florida,[19] and began distancing himself from the other Yale researchers. He now published under the patronage of the Yale Laboratories

[18] A third female student, Dorothea McCarthy, co-authored on Lashley's 1926 paper dealing with cerebral injuries in rats. Since she was second author, however, she was not present in the author-organism dyad. Shirley and Hubbard took their PhDs studying experimental psychology with Lashley. McCarthy, along with another female student, Esther McGinnis, took their PhDs from the Institute of Child Development run by Anderson and Goodenough (About the Institute, 2013).

[19] Yerkes considered this a three-point operation that included a facility in New Haven and fieldwork overseas. On June 10, 1930, a day after the facility was built, four chimps moved in from Yale.


human

of Primate Biology until he retired in 1942. Dunlap was at Johns Hopkins until moving to UCLA in 1936 to take up a position as Professor and Chairman of the Department of Psychology, remaining there until retiring in 1946 (Moore, 1959). Lashley held multiple positions in this period: he was at Chicago from 1929 to 1935, Harvard from 1935 to 1955, and at the YLPB from 1942 to 1955. Thus, this section focused on the YLPB, Chicago, Johns Hopkins, Harvard, and UCLA.

Researchers at the YLPB began to publish under this affiliation in the *JCP* in 1934. From 1934 to 1941, while the YLPB was under the control of Yerkes, 35 articles were published. From 1942 to 1950, under Lashley's leadership, only 14 articles were published. Also in 1942, the YLPB became a joint venture between Yale and Harvard (Dewsbury, 1984); no doubt due to Lashley's incumbency at Harvard. In total, 49 articles came from the YLPB in this journal, 40 of which were on the chimpanzee. The YLPB was not only the most prolific primate laboratory of the era in terms of articles, but by 1941, the Yale Laboratories of Primate Biology (later the Yerkes Laboratory of Primate Biology in 1942) would boast the largest collection of chimpanzees anywhere at forty-five (Gray, 1955; Hahn, 1971). The other nine articles were written across six different species (humans, rats, minnows, octopus, guinea pigs, and pigeons).

While at Johns Hopkins in this time period, Dunlap only studied the rat. Upon his move to UCLA, he returned to studying the human once again. During this time in the *JCP*, it was rare for a researcher to distinguish between the Berkeley and Los Angeles campuses of the University of California: the majority of researchers characterized their affiliation as being with the "University of California." To avoid misattributing a researcher to the wrong campus, I decided to record all instances as the University of California. Thus, it is impossible to determine Dunlap's influence in the overall publishing trends of the institute. However, considering the

goal of his move was to establish a psychology laboratory and graduate program, I would presume that Dunlap was tied up with more administrative tasks. It is interesting to note the sudden drop off in productivity from Johns Hopkins. In this time period, Hopkins researchers published 33 articles on six species; the seventh most productive laboratory in terms of number of articles. Compared to the 62 published in the previous time period, the Hopkins laboratory productivity dropped by half in a time when the total number of articles being published in the *JCP* was increasing. This might have been due to Dunlap's departure in 1936 (who remained editor until 1943), or by the growing influence of the more behavioristically inclined institutions (this will be discussed in more detail in the fourth chapter).

Although Lashley only published two articles on the rat while at Chicago, he appeared to have left his mark. Throughout the period of 1911 to 1950, Chicago had always been near the top in both degree and weighted degree. This was mostly due to the organizational skill of Harvey Carr. Under his guidance, Chicago served as one of the most prolific psychology departments of any university, which took a functionalist and comparative approach in their research (Carr, 1927; Pillsbury, 1955). However, Lashley's research in the 1930s revolved around pattern perception and his mixture of behavioral and anatomical methods subsequently resulted in the increase of papers dealing with the rat's brain mechanisms coming out of the Chicago laboratory. His lasting contribution to the institution, however, came from his influence on future Chicago research, and his specific impact on two individuals, David Krech and Yu-Chuan Tsang. Upon receiving his doctorate from Berkeley in 1933, Krech went to Chicago with a National Research Council Fellowship to work alongside Lashley, and stayed until 1937. In the years Krech was at Chicago, he served as Lashley's assistant and published six articles on the rat; the most any

researcher had at Chicago from 1911 to 1950 in the *JCP*.[20] Yu-Chuan Tsang was much less prolific than Krech, publishing only three papers on the rat; two in 1937, and one in 1938. However, in each of his articles, in a footnote, Tsang acknowledged that he "is indebted to Professor K. S. Lashley for suggestions and encouragement" (Tsang, 1938, p. 1). Lashley's influence remained for years after he had left in 1935.

Most interestingly, however, was the pattern Harvard research took in this period. After Yerkes had left Harvard, in 1917, the department began to fade in terms of its productivity in the *JCP*. Indeed, only five of the institutions 29 articles came between 1918 and 1930. Furthermore, the institution did not publish a single article from 1931 to 1936. With the arrival of Lashley to Harvard in 1935, the psychology department was revived. Lashley, himself, did not publish an article while affiliated with Harvard. However, two patterns suggest that the Harvard laboratory was influenced considerably by Lashley. First, an institution that had long been established as comparative became suddenly invested in a single organism: the rat. Harvard's degree of three in this period was a far cry from its degree of 20 in the previous period. Moreover, of the institutions 26 articles, 23 were on the rat, the organism that Lashley had long determined as his "standard." Second, Harvard researchers began publishing in the *JCP* again in 1937, the year Lashley obtained his coveted role of research professor in neuropsychology. Furthermore, these researchers publishing in 1937 were former students of Lashley's, Frank Beach and Donald O. Hebb, who followed Lashley to Harvard (Dewsbury, 2002).

This chapter sought to test Dewsbury's claim that comparative psychology did not just fade into behaviorism, but maintained a consistent thread of excellence throughout its history. In particular, this chapter focused on the first generation[21] of comparative psychologists, Robert

[20] Krech published under his birth name, Ivan Krechevksy.
[21] By no means is this a suggestion that comparative work did not begin until these three arrived.

Yerkes, Karl Lashley, and Knight Dunlap, that Dewsbury credits with institutionalizing comparative psychology and sustaining it through a period of transition (in the 1920s). To determine this, I analyzed the data collected from the *JCP* that was displayed in a network visualization, looking specifically at the degree, weighted degree, and positioning of Yerkes, Lashley, and Dunlap. I determined that all three of these comparative psychologists were among the most comparative and prolific contributors to the *JCP,* and were each positioned as conduits near the center of the network, connecting and mediating the flow of information between multiple communities of researchers and organisms. I also analyzed these three individuals at the level of their institutional affiliation. Here, I found that the institutions employing their respective psychobiologist adopted their distinct approach and standard laboratory organism. Moreover, these institutions were amongst the most active contributors to the *JCP*. Another finding came from the overlap and intertwining of these institutions. The leading psychobiologists were affiliated with a small handful of institutions in which they moved around and replaced each other. That Lashley replaced Yerkes at Minnesota and Orange Park was not accidental, but instead part of a larger initiative to continue the psychobiological tradition.

Thus, Dewsbury's assertion that a thread of excellence was maintained in comparative psychology due to a handful of individuals was quantified through the networks. A further influence of Yerkes and Dunlap that cannot be explicitly seen in the network (can only be inferred) were their positions as editors of the journal: Yerkes from 1911 to 1916, and Dunlap from 1917 to 1943. The next chapter assesses the second part of Dewsbury's claim that comparative psychology prospered in the 1930s and 1940s off the achievements of a newly graduated crop of comparative psychologists.

Chapter Two: New Generation of Comparative Psychobiology

The argument in this chapter that comparative psychology prospered on the backs of a "new generation" of comparative psychobiologists rests on the premise that the comparative psychobiologists discussed here were indeed psychobiologists. Although some of the new crop of comparative psychologists were characterized by Dewsbury (2002) as psychobiologists (Beach and Maier), the majority have rarely been titled as such. Thus, to show that these individuals could be constituted as psychobiologists, I used Yerkes' and Dunlap's integrative definition as the determining criteria: that the researcher employed some intersection between psychology and biology in their study of behavior, and that they did not subscribe to the prevailing behavioristic framework of the day.

Some of the individuals that made up Dewsbury's (1984) "new generation" of comparative psychobiologists were, in chronological order, Calvin P. Stone, Norman R. F. Maier, Harry F. Harlow, Winthrop N. Kellogg, and Frank A. Beach. My criteria for inclusion were as follows: the scientists had to have been identified by Dewsbury as important comparative researchers; and each scientist must have contributed at least ten first-author publications to the *JCP* from 1911-1950. Similar to the previous chapter, my argument will be constructed as follows: it begins with a brief biographical sketch of these researchers, illustrating how these individuals were indeed psychobiologists. Next I assess their influence at the individual and institutional level in the *JCP* using the measures of degree, weighted degree, and overall positioning in the network. The more active a researcher in the *JCP*, the more influence that individual was likely to have been in contributing to the growth of the comparative discipline. This belief was integral to the interpretation of the data.

Biographical Sketch of the "New Generation"

Calvin Perry Stone entered the comparative sphere the earliest amongst the five "new" comparative psychobiologists. After receiving his BS in 1910 and BA in 1913 in different departments at Valparaiso University, and a brief stint as a high school teacher, Stone moved to Indiana where he received his MA under Melvin E. Haggerty in 1916. Stone followed Haggerty to Minnesota, but his graduate career was interrupted by The Great War. After the war, he returned to Minnesota where he was primarily influenced by Lashley, taking his PhD under Lashley's supervision in 1921 (Rosvold, 1955; Dewsbury, 1984; Hilgard, 1994). Stone's dissertation was titled, "An experimental analysis of the congenital sexual behavior of the male Albino rat." By this time, Lashley was a seasoned rat researcher, so it is no surprise that Stone moved towards rat research. In fact, Lashley suggested the problem to Stone that materialized into his dissertation on the copulatory behavior of male rats (Hebb, 1959).

The next comparative psychobiologist to earn his doctorate was Norman Raymond Frederick Maier. Maier began his studies in Michigan where he received his BA at the University of Michigan in 1923. After, he travelled abroad for a year of graduate work in 1925 and 1926 to the University of Berlin where he studied under Wolfgang Köhler, Max Wertheimer, and Kurt Lewin. Upon his return to Michigan, he completed his doctorate under the supervision of John F. Shepard, before moving to join the faculty at Long Island University in 1928 and 1929. From there, he became a National Research Council Fellow at the University of Chicago between the years 1929-1931, where he was afforded the opportunity to work with Karl Lashley and Heinrich Klüver. At Chicago, Maier began his research studying reasoning in humans (Maier, 1930) and guinea pigs (although not in the *JCP*), before moving to problem solving in rats. In 1931, Maier returned to Michigan, where he held a faculty position for the majority of his career (Dewsbury, 1984; Solem & McKeachie, 1979).

Winthrop Niles Kellogg was next to enter the discipline. He first attended Cornell University in 1916 and 1917, but like many of his compatriots, he left to serve in the war. At the war's end, he enrolled in Indiana University where he obtained his degree in philosophy and psychology in 1922. Kellogg subsequently went to Columbia, receiving his MA in 1927, and doctorate in 1929 under the supervision of Robert S. Woodworth. His dissertation, which compared psychophysics methods, characterized his early research (Deese, 1973). Kellogg joined the Indiana faculty in 1929, and first began publishing in the *JCP* in 1931; a study on fear behavior in rats, birds, and mice. He spent the majority of the 1931-32 academic year at the Orange Park facility, where using a research leave funded by the Social Science Research Council, he conducted his most (in)famous study, *The Ape and the Child* (1933). In this controversial developmental study, Winthrop raised his son, Donald, alongside a chimpanzee named Gua. Gua was obtained from the YLPB on June 26, 1931 when Donald was nearly one year old, the experiment lasting until March 28, 1932 (Dewsbury, 1984; Benjamin & Bruce, 1982). He returned to Indiana in 1933, and published learning studies on humans (1935) and water snakes (1936) in the *JCP*, before choosing the dog as a subject. This coincided with Kellogg's establishment of a special dog conditioning laboratory in 1936 (Kellogg, 1938; Benjamin & Bruce, 1982). The remainder of Kellogg's work at Indiana revolved around conditioning and learning in dogs.

Harry Friedrich Harlow[22] was directly connected to both Lashley and Stone. Stone, who took his doctorate under Lashley, supervised Harlow during his PhD. Harlow spent his entire graduate education at Stanford, obtained his BA in 1927, and PhD in 1930. Although Stone was Harlow's supervisor, Harlow was also influenced by Lewis M. Terman and Walter R. Miles who

[22] Harry Harlow was born Harry Israel but was encouraged by Terman to change his name to improve his job prospects. Although he was not of Jewish descent, his last name would cause people to assume he was (Rumbaugh, 1997).

were both at Stanford at the time (Sears, 1982; Rumbaugh, 1997; Dewsbury, 1984). Like his supervisor had done to him, Stone attempted to direct Harlow toward rat research early on, assigning him a "pedestrian rat problem [that] soured [Harlow] forever on both rats and parametric research" (Sears, 1982, p. 1280).[23] Harlow was brought in to the University of Wisconsin in 1930 to take up a position as comparative psychologist and director of the animal laboratory. Upon his arrival, Harlow discovered there was no such laboratory (Sears, 1982; Rumbaugh, 1997), and instead, began studying primates, including the monkey, at the Madison Zoo. There are two different accounts describing this event (Rumbaugh, 1997). The first recounts that Harlow, an accomplished bridge player, was given the suggestion of studying primates at the Madison Zoo by his department chair's wife during an evening of cards (Rumbaugh, 1997). The second account credited the suggestion to Clara Mears, a teaching assistant in educational psychology, and Harlow's future wife in 1932 (Sears, 1982; Rumbaugh, 1997). In any case, to avoid working with rats, Harlow began research on primates at the Madison Zoo; he began with apes, baboons, lemurs and orangutans, before choosing the monkey (Sears, 1982; Rumbaugh, 1997).

The last of the new generation of comparative psychobiologists discussed here was Frank Ambrose Beach. Beach took his undergraduate training in English in 1932, and then took his Master's in clinical psychology at the Kansas State Teachers College. His MA thesis was on color vision in rats, and was developed from his own ideas. He moved to Chicago for a year of graduate school on the promise of a $400 stipend from functionalist Harvey Carr. At Chicago, he came under the influence of Carr, L.L. Thurstone, and Lashley. With his fellowship at Chicago complete, he taught at a high school in Kansas, before returning back to Chicago in 1935 to

[23] Harlow did not start publishing in the *JCP* until 1932, and thus, none of his work at Stanford appeared in the journal.

finish his studies. By that time, Lashley had left for Harvard and Beach would eventually follow him there after the completion of his doctoral dissertation in 1936. Although Beach completed his doctoral dissertation on the cortical control of maternal behavior in rats while at Chicago in 1936, he did not actually take his doctorate until 1940 when he was stationed at the American Museum of Natural History. Beach spent the academic year of 1936/37 working in Lashley's laboratory at Harvard and, in 1938, he took a position at the American Museum of Natural History (AMNH) in the Department of Experimental Biology. Beach remained there until 1946 before leaving to Yale, and eventually Berkeley (Dewsbury, 1989; Dewsbury, 1984). His time at the AMNH exposed him to the comparative study of organisms other than the rat, which perhaps led to his critique of psychology's overuse of the rat (Pettit et al., forthcoming/2015).

In the previous chapter, we saw several commonalities between Yerkes, Lashley, and Dunlap. The most important of which were the similarities in laboratory organisms studied and the comparative approach each employed. The five psychobiologists in this chapter also shared many of the same laboratory organisms: Stone, Maier, Kellogg, and Harlow studied the human subject; Stone, Maier, Kellogg, and Beach experimented on the rat; and Stone and Kellogg both conducted research on the dog and the house mouse. However, three distinct experimental approaches emerged from these five researchers. Stone, Kellogg, and Harlow took a more comparative approach, studying multiple organisms early in their careers before settling on a model organism. Maier took a more narrow approach in terms of the diversity of his study species. And Beach began his career focused solely on one species before employing a more comparative approach later on in his career (beyond the time period of this thesis). Indeed, the comparative tag attributed to Maier and Beach by Dewsbury (1984) referred more to their influence on the comparative discipline and less to the diversity of organism used in their own

research. Nonetheless, the two were still amongst the most active contributors to the *JCP* and influential in sustaining the discipline. In fact, Maier was the most active of all researchers, conducting 25 studies from 1930 to 1950. See Table 2 for the fifteen most comparative and productive authors.

All five of these psychobiologists were highly active in the *JCP* from 1930 to 1950, publishing at least ten articles each. However, only Stone was publishing prior to 1930. Stone, Kellogg, and Harlow featured as the most comparative researchers (studying the most diverse organisms) behind only Yerkes. Harlow was the second most comparative studying seven organisms (human, cat, monkey, ape, lemur, orangutan, and baboon), while Stone (human, rat, dog, mouse, horse, rabbit) and Kellogg (human, dog, rat, mouse, bird, and snake) tied for the third most comparative at six. Maier (rat and human) and Beach (rat) studied two and one organism, respectively. Like their predecessors, all five psychobiologists had their preferred model organism. Stone, a student of Lashley, predictably favored the rat, publishing fifteen articles with it as subject. Maier, who also worked under Lashley, chose the rat for his studies on reasoning – which Maier believed was distinct from learning – publishing 22 of his 25 articles on the rat. Kellogg, who conducted much work on conditioning (Benjamin & Bruce, 1982), published six of his ten articles on the dog. Harlow preferred the rhesus macaque amid his primate subjects, conducting nine of his twelve studies on the animal. And finally, Beach, inspired by Carr and Lashley, experimented on the rat in all eleven of his studies. Moreover, each of these five "found" their standard organisms in different ways. Stone (1921) conducted his MA thesis on light discriminations in dogs, but quickly moved to the rat. He studied the other organisms intermittently throughout his career. Maier began his career alternating between the human and the rat before settling solely on the rodent. Harlow and Kellogg took a similar pattern

in finding their model organism: both started with a comparative approach before choosing the monkey and dog, respectively. Beach, a product of Carr and Lashley at Chicago, chose the rat from the start. Ironically, Beach, who favored the rat, was perhaps best known for his critique that the discipline had become over-reliant on the animal, criticizing his own research in the process. Later on in his career, Beach employed a more comparative approach.

Determining a Psychobiologist

As mentioned above, the argument in this thesis is contingent on the characterization of these five individuals as psychobiologists. Otherwise, it would be impossible to distinguish between a rat researcher operating under a behavioristic framework and one operating outside the behavioristic paradigm. Moreover, it is important that this characterization be spelled out prior to the network analysis. To determine whether or not these individuals were psychobiologists, I employed Yerkes' and Dunlap's definition of the term which states that 1) behavior must be studied at the biological and psychological level and 2) the research problems be approached from non-behaviorist theoretical viewpoint. To be characterized as a psychobiologist, the researcher must have adhered to both aspects of the definition. This appeared to be the case. The first individual I assessed was Calvin P. Stone.

Stone was the most blatant advocate for the biological aspects of behavior of the five. For Stone, understanding behavior included studying its genetic basis, development, and control at the endocrine level. Such a strong predisposition to the biology of the organism was expected considering Stone's background in physiology and anatomy. For certain, having Lashley as a supervisor,[24] whom many considered a physiological psychologist (Hebb, 1959), ensured Stone's

[24] In the past, many considered Lashley only a "physiologist." Although he trained primarily in biology and his work entailed neurology and neurophysiology, Lashley considered himself a psychologist foremost and was most interested in the study of behavior (Hebb, 1959). Some historians now recognize him as a psychobiologist (Dewsbury, 2002).

training in the physiological mechanisms of behavior. Additionally, while at Minnesota, he was also influenced by the anatomy professor, A.T. Rasmussen (Rosvold, 1955; Hilgard, 1994; Dewsbury, 1984). Stone's early work highlighted instinctive behavior and the relationship between physiological mechanisms and reproductive behavior; work he was considered the pioneer of (Dewsbury, 1984). Later in his career, Stone moved more towards the study of maze learning, and drew attention to the importance of studying abnormal behavior. Stone, like Lashley, could have always been considered a physiological psychologist, but with his later work on the effects of electroconvulsive shock on learning, he moved closer to the physiological side. Perhaps it is coincidental, but he also took on executive editorship of the *JCP* when its name was changed to include "*Physiological Psychology.*"

Although Stone ventured into studies on learning behavior, he was far from a behaviorist and never wavered from his physiological psychological research program. In fact, Stone is remembered for his uncompromising stance that psychologists needed to stick to the "well-grounded fundamentals," and his ability to remain undeterred by the pedagogical trends associated with behavioristic theory (Rosvold, 1955, p. 328; Dewsbury, 1984) Further separating him from the behaviorisms of Hull and Skinner was his visible promotion of the study of instincts, culminating in the "Symposium on Heredity and Environment" held on April 10, 1947 in conjunction with the meetings of the Society of Experimental Psychologists at Princeton. Stone joined the likes of Beach, Leonard Carmichael, Lashley, and Clifford T. Morgan as speakers, while Walter S. Hunter served as discussant. All the papers, published in the *Psychological Review*, argued for the existence of instincts. Stone's participation in this symposium, coupled with his APA presidential address in 1942 entitled, "Multiply, Vary, Let the Strongest Live and the Weakest Die – Charles Darwin," and his studying of maternal behavior

and other instinctual behavioral processes, led to the characterization of Stone as a psychobiologist and his research program as comparative psychobiological.

Norman Maier had already been characterized by Dewsbury (2002) as a psychobiologist, and his research program supported this assertion. Like Stone, he was also highly influenced by Karl Lashley, their careers intersecting during Maier's fellowship at Chicago from 1929-1931. Lashley's influence surfaced in Maier's use of the Lashley Jumping Stand and his study of behavior at the neural level. Using the Lashley Jumping Stand, a device that required rats to jump from a small platform to the target area, Maier (1939) claimed to have discovered two forms of neurotic behavior in rats. After the rats learned a simple discrimination, they were presented with an unsolvable problem and displayed two different behaviors: some became fixated with a particular choice that when presented with a solvable problem, they continued to make that same choice; and others experienced full-fledged seizures that sometimes lasted several minutes. Maier believed both behaviors were similar to the types of neuroses prevalent in humans, but later conceded that only the fixations were indicative of neurotic behavior (Maier, 1939; Dewsbury, 2010; Dewsbury, 1993). Maier, who was already considered a Maverick in psychological circles, drew the ire of the behaviorists with this research, especially with the public support it was given being awarded the $1000 Newcomb Cleveland Prize; the first psychologist to be given this award by the American Association for the Advancement of Science (for a full account, see Dewsbury, 2010). His research ran counter to the dominant behavioristic trends of the day and, for this reason, it was attacked by mainstream psychologists. Although Maier successfully defended his theories, his research was not accepted and he was later forced out of academia. He became a successful industrial psychologist (Dewsbury, 1993).

Kellogg's approach to his research might be described as "Natural Curiosity." Of the five, Kellogg was the most likely to have been a behaviorist, considering his work on conditioning and learning in multiple organisms. However, Kellogg never adopted such a framework. In fact, it was this atheoretical position, along with his personality, that put him in such a precarious situation at Indiana. Kellogg spurned theory, arguing that it blinded researchers from seeing important findings or misinterpreting results. For him, science belonged to natural curiosity. At a time when researchers adhered to one learning theory or another, Kellogg's position was unpopular; not only at Indiana, but at many institutions in the 1930s and '40s. Moreover, with behaviorism looming large in learning studies, Kellogg's curiosity was akin to Titchener's science for science's sake; perhaps something he learned while at Cornell in 1916. Kellogg, opposed to behavioristic theory, was also actively involved in researching the biological aspects of his organisms. His famous study with Gua the chimpanzee focused on the development of the primate, and his later work on echo localization in the dolphin's sonar system was inherently biologically based. Thus, like Stone and Maier, Kellogg was characterized as a psychobiologist.

Harlow, a product of Stone and Stanford, also benefitted from a strong background in physiology and neuroanatomy. As such, his early interests were in the cortical localization of higher intellectual functioning (Sears, 1982). It was this focus on "higher" intelligence that contributed to his choice of monkeys as subjects since they evidently showed high cognitive capacity. This early focus on the biological aspects interested him, but not nearly as much as his serendipitous finding of "learning sets,"[25] the process by which an organism learns to learn (Sears, 1982, p. 1280; Dewsbury, 1984; Rumbaugh, 1997). In the process of constructing his Wisconsin General Test Apparatus standard battery – the standardized set of measures he needed

[25] Wolfgang Köhler and Robert Yerkes similarly found that their primates learned to learn (Rumbaugh, 1997).

to examine higher intelligence at the central nervous system level – he witnessed his monkeys displaying one-trial learning on simple discrimination problems. First, they responded through trial-and-error fashion, much the way conventional learning theory indicated, but eventually, the monkeys "caught" on to a shortcut in the process, using insight to display one-trial learning. This finding sent shockwaves throughout the discipline, forcing behaviorists to develop more cognitive theories. Indeed, Sears (1982) considered it his "first victory in his battle with the Yale [Hull] brand of behavior theory" (p. 1280). Interestingly enough, Harlow only discovered the monkey's ability to learn to learn because the depression of the 1930s meant he only had access to a few monkeys and had to use the same ones in successive trials.

A second finding of Harlow's threatened to dismantle the idea of primary drive reduction as a reinforcer that was at the center of Hull's behaviorism. In a study in which monkeys were well-fed, Harlow found that curiosity was a stronger motivator than hunger, a finding that was earlier purported by Lawrence Cole, an MA student of Yerkes, with his work on raccoons (Pettit, 2010). Harlow set up a chamber in which monkeys were given an opportunity to work a gadget in order to open up a peephole to see a toy electric train on a track. He found that the monkeys worked for hours on difficult discrimination problems to witness this real life toy in action. Yet, Harlow was perhaps best known for his work on the development of affectional systems in monkeys. This research "included the development of 'surrogate mothers' and studies of mother-infant relationships, sibling relationships, abnormal behavior, and therapy" (Dewsbury, 1984, p. 302). This brought Harlow back to his biological roots, but also came from his on-going effort at dismantling primary-drive theory. Again, this finding was serendipitous and came from his attempt to start his own breeding colony of rhesus macaques. Harlow separated the infants from their mothers for fear of spreading any pervasive diseases that the mothers brought with them

from India. He found that the infants thrived physically, but were deprived emotionally and refused to mate as adults. This led to his "surrogates," the wire and cloth mothers (Sears, 1982).

Harlow's commitment to the biological processes, and his obvious disbelief in behaviorism, not only made him a psychobiologist, but led Rumbaugh (1997) to characterize his theoretical approach as "rational behaviorism" because his theory was "one that was to tumble the Jerichonian walls of Thorndike's bondage…[and] the habit strength of Hull and his drive-reduction framework" (p. 202, 204).

Similar to Maier, Dewsbury (2002) had already characterized Frank Beach as a psychobiologist. And like Stone and Maier before him, Lashley's mentorship was pervasive in Beach's career, influencing his organism choice and his research program. Beach first studied under Lashley when he was brought to Chicago by Harvey Carr, and like many of Lashley's students, he eventually followed his mentor to Harvard in the late 1930s. Carr and Lashley put Beach onto the rat as a research subject early on in his career. However, Beach's rat research was of the psychobiological variety, dealing with reproductive behavior at the endocrine and neural level (Dewsbury, 1984; Dewsbury, 1989; Pettit, 2012). Further purporting the importance of biology in his research program, Beach is credited as a major contributor to the establishment of behavioral endocrinology as an important field of inquiry.

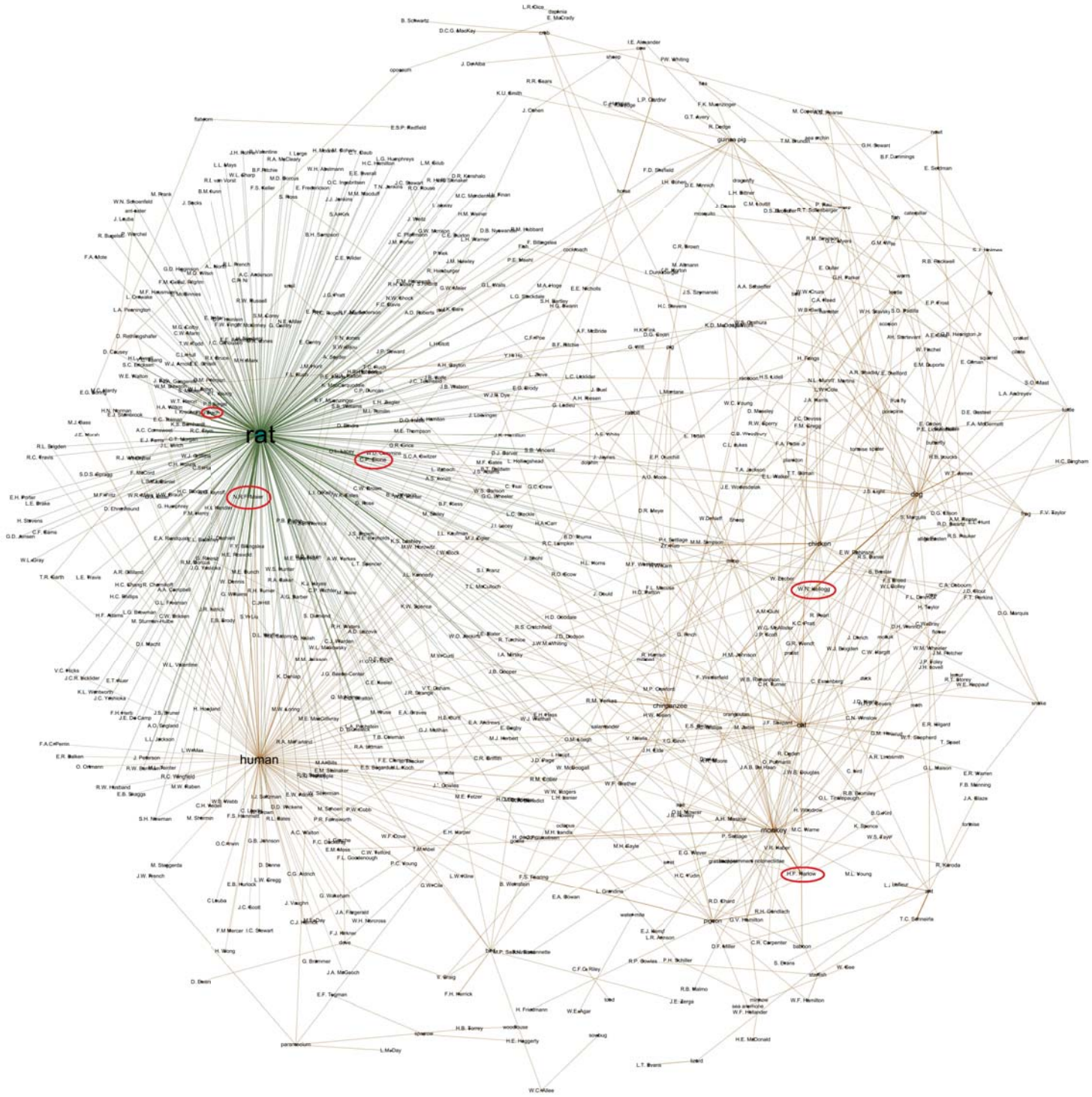
Beach's opposition to behaviorism was highlighted by his paper entitled, "The Snark was a Boojum" in 1950. He did not so much oppose the behavioristic framework for its inherent defects, like Harlow did, but rather opposed its displacement of comparative psychology. Beach feared that behaviorism had turned comparative psychology into the study of maze learning in rats, and argued that if change was not enacted, the comparative discipline would soon disappear. Thus, based on Yerkes' and Dunlap's definition of psychobiology, it is safe to characterize these

five researchers as psychobiologists. The establishment of this characterization was an integral part of the overall argument, and provided the necessary foundation to make any claim about the influence of these researchers within network and by extension, the overall discipline. The next section investigates the influence of these individual comparative psychologists in the network.

The New Generation in the Network

It is clear from the biographical sketches that all five of these authors were highly active contributors to the flagship journal of the discipline. And with the exception of Beach and Maier, these psychobiologists were some of the most comparative researchers around. Thus, strictly in terms of quantity, it would appear that these five researchers were highly influential in helping comparative psychology "prosper" in the 1930s, and maintain its "vigor" in the 1940s. This next section sought to assess their influence in the structure of the network through their positioning. As was seen in the previous chapter, a researcher's position in the network is highly influenced by their degree and weighted degree. The higher one's degree, the more connected the researcher will be, and will likely position them closer to the center of the network. The closer one is to the center indicated that that researcher was likely to have been mediating the flow of information between multiple organism communities. Moreover, a high instance between a researcher and organism (multiple studies on the same animal) determines the spatial closeness of those two representative nodes in the network.

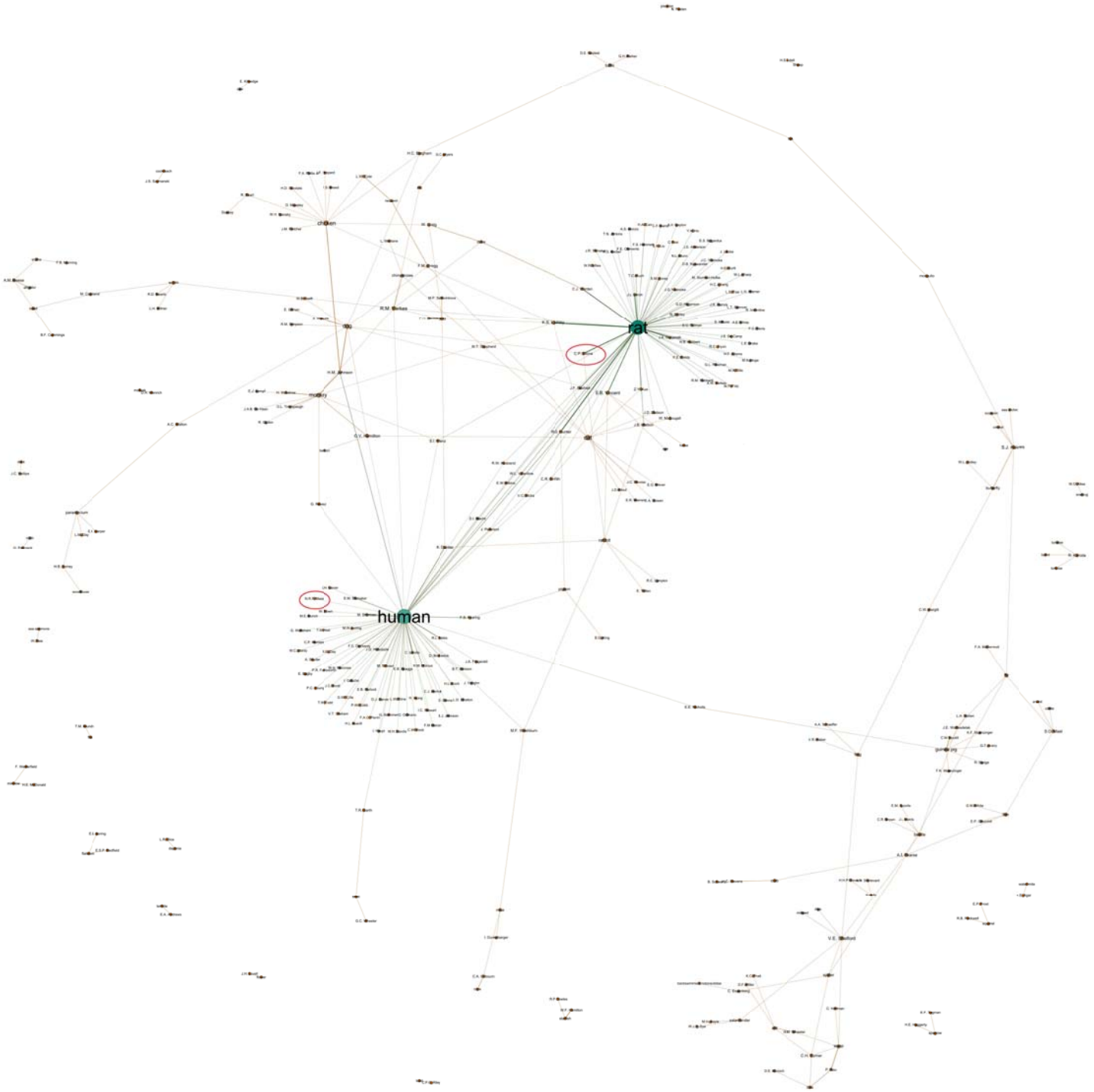
Figure 7 depicts the same time period from 1911 to 1950 with these five psychobiologists circled. Stone (d=6, wd=21), Maier (d=2, wd=25), and Beach (d=1, wd=11) were all predictably closest to the rat. Stone, being the most comparative of the three, was positioned closest to the center (on the right side of the rat). Beach, who only conducted studies on the rat, was furthest from the center (left side of the rat), and positioned more on the periphery. However, Beach's


rat
human
dog
chicken
monkey
W.K. Kellogg
H.F. Harlow

eleven studies on the rat made him one of the spatially closest researchers to the rodent. Maier, who also studied the human, was positioned at the bottom of the rat node, and like Dunlap, acted as a conduit between the two largest communities of researchers. Harlow (d=7, wd=15), to no surprise, was closest to the monkey. Kellogg (d=6, wd=11), however, was noticeably positioned closer to the mouse– in which he conducted only one study – than the dog. This appeared to be caused by his connection to the bird, located at the bottom of the network. In contrast to Stone, who was positioned closer to the center, Harlow and Kellogg were positioned closer to the edge of the network. This was no doubt due to their minimal engagement with the rat and human subjects. The fact that the rat was located at one end of the network, and the monkey and dog at the other end, reflects that very few researchers who studied rats were also studying the other two organisms. Indeed, Beach (1950) argued that most researchers studying the rat were not studying other organisms at all. Again, I looked at the network diachronically, splitting the time period from 1911 to 1930 and from 1931 to 1950.

The Early Years (1911-1930)

Since this chapter focuses on the new generation of comparative psychobiologists, it is unsurprising that only two of the five psychobiologists were publishing in this time period. Figure 8 displayed the network from 1911 to 1930 with Stone and Maier circled. Stone published eight articles in this period, six on the rat, and one each on the dog and rabbit. Maier only published one article on the human, and thus, was positioned on the far left side of the network. Stone's first publication on light discrimination in dogs came in 1921, and thus, his eight articles came over a span of ten years. Since the majority of his work was on the rat, he was closest to its cluster. Notice how Stone appeared on the left side of the rat cluster rather than the right. The researchers on the rat side were those only studying the rat. However, since Stone was also

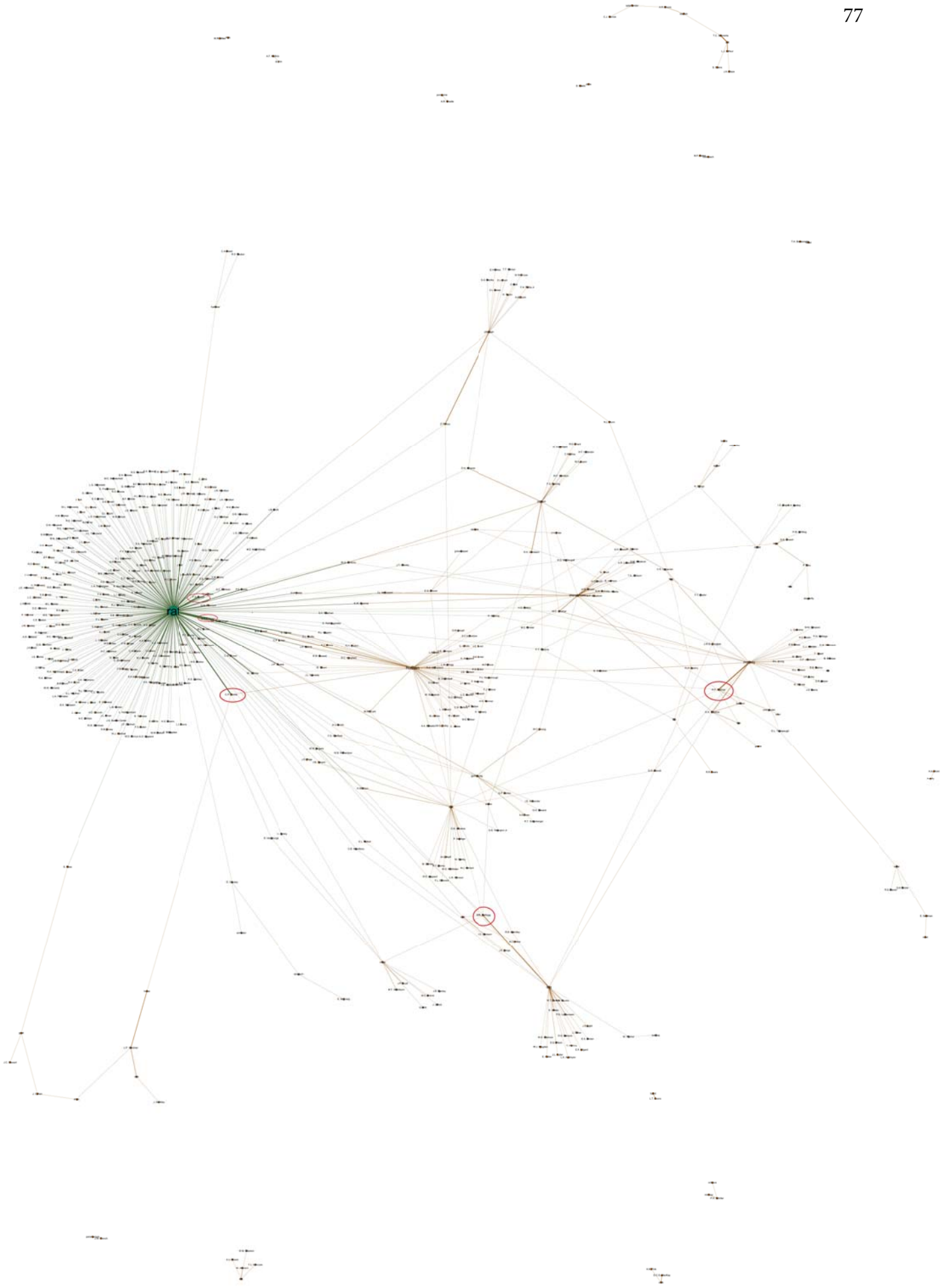

rat
human

studying the dog and rabbit, he was positioned on the left side and acted as a liaison to the dog and rabbit nodes. He was also located in the main part of the network (in between the rat and human clusters), asserting his influence in the network's structure.

The Later Years (1931-1950)

The second network, from 1931 to 1950, with all five authors circled, was depicted in Figure 9. Stone, who was prominent in the earlier network, was even more productive and influential in this time period: he conducted thirteen studies, nine on the rat, two on the human, and one each on mice and horses. Whereas in the last period Stone's close connection to the rat brought him closer to the center, in this period, his close connection to the rat pulled him further from the center. The network, in this period, was partitioning into two parts, the rat researchers, and a collection of other clusters of various sizes. However, Stone, like Dunlap, was one of the few individuals connecting the rat and human cluster, so he did not fall too far from the center. Stone was also one of three researchers studying both rats and mice – the others being R. Heimburger and I.A. Mirsky. Stone's structural prominence in the network was also attested to by his connection to the horse. The horse was being studied by two individuals in this period: Stone and L.P. Gardner. Gardner, who also studied cows and sheep, was not connected to the main network. Thus, Stone was the sole reason for the direct connection of the horse to the main network.

Norman Maier conducted 24 studies in this time frame: two on the human and 22 on the rat. Maier was the most productive of any of the researchers, and his many studies on the rat made him one of the closest to the cluster. However, his two studies on the human kept him firmly positioned on the right side of the rat cluster, and once again, positioned him as one of the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handful of researchers that mediated the two largest communities of researchers. Maier's influence was mostly seen in how productive he was in this period.

Harlow's comparative work on multiple primates, coupled with no connection to the rat, led to his location on the far right side of the network, and separation from the main of the network. In terms of statistics, Harlow was the most comparative psychologist in this period, studying seven organisms; closely followed by his student, Abraham Maslow (who studied at the Madison Zoo with Harlow), and Kellogg, who each studied six species. Harlow was also tied as the second most eminent psychologist with a weighted degree of 15 – behind only Norman Maier's staggering 24. Marion Bunch also had a weighted degree of fifteen. Harlow's location near the edge of the network seemed to dismantle the argument that a high degree positions a researcher near the center; however, this was not the case. With no connection to the rat – the main part of the network – and the fact that he was studying nearly all the primates that appeared in this journal, he created and centered his own primate community within the network. Moreover, the majority of monkey researchers were studying only the monkey and were, accordingly, positioned on the right side of the organism. Harlow, who was studying humans, lemurs, orangutans, apes, baboons, cats and monkeys, was located on the left side of the monkey, or what can be termed the "comparative side." Harlow's nine publications on the monkey also made him the foremost monkey researcher in the network. Yet, he was closest to the baboon and ape. This was easily explained: since the baboon and ape had low degrees, three and two respectively, they were more likely to be influenced by the much more prominent Harlow (degree of 7), and thus, be drawn closer to his representative node. Further purporting Harlow's importance in the network, he was the only researcher in the entire network that conducted studies on orangutans and lemurs, and was the reason the two primates appeared in the network.

He was also one of two individuals, the other being B. Weinstein, who connected both the monkey and human clusters, and the only primate researcher connected to the cat cluster.

All of Kellogg's work came in this period. His ten articles on six species were broken down as follows: six on dogs, and one each on the rat, human, mouse, bird, and snake. Kellogg's location in the lower middle part of the network was due in most part to his high occurrence with the dog; the dog cluster was located at the very bottom of the network. Indeed, alongside W.T. James, Kellogg conducted the most studies on dogs of any other researcher. Similar to Dunlap, Stone, and Maier, Kellogg was also a mediator between the rat and human cluster. However, with only one article on each organism, he was not similarly positioned in between the two clusters. The closest species to Kellogg was the bird. As was seen in the case with Harlow, the bird, with a smaller degree of five, was drawn closer to the larger degree of Kellogg's (6). The Indiana psychologist was also connected to the mouse and the snake. Kellogg's prominence in the network was also seen in the idiosyncratic connections he made: he was the only researcher studying dogs and mice; and one of only three researchers connecting the rat and dog clusters (the other two being E.L. Walker, C.B. Woodbury).

Of all the psychobiologists, Frank Beach probably exerted the least influence on the overall structure of the network. However, his eleven studies of the rat did make him one of the foremost rat researchers of the day. There is a caveat when assessing Beach's influence in comparative psychology during this period. Beach was the youngest of all the psychobiologists discussed in this thesis, and thus, did not start publishing in the *JCP* until 1937. Moreover, most of his comparative research did not begin until after 1950 when he had left the American Museum of Natural History. During his time at the Museum, he became interested in European

ethology and served as a conduit between it and American comparative psychology after the Second World War.

Dewsbury's argument appeared to have been supported not only by the quantitative measures of the new generation of psychobiologists, but also by their positioning in the network. Even though Maier and Beach were not as comparative as Stone, Kellogg, and Harlow, their productivity made them two of the most eminent rat researchers in the entire discipline that adhered to a psychobiological framework instead of the dominant behavioristic paradigm. Stone, Kellogg, and Harlow represented the new generation of truly comparative psychobiologists that maintained the tradition of studying a diversity of organisms in an era when the rat was the preferred model organism in many scientific disciplines. These three were also amongst the most active contributors to the journal. Thus, these five individuals did indeed help comparative psychology prosper in the 1930s, and prosper the discipline did: from 1911 to 1929, 366 articles were published in the *JCP* compared to the 1040 published from 1930 to 1950; an increase of nearly threefold. The next section investigated the influence of these individuals from the institutional level.

Institutional Affiliation of the New Generation

There were several commonalities among these five authors. Both Stone and Kellogg began their graduate careers at Indiana. After taking his MA, Stone moved to Minnesota for his PhD, before joining the faculty at Stanford where he would spend the rest of his career. Kellogg completed his master's and doctorate at Indiana, and then took a faculty position there that he held until 1950 when he moved to Florida State University. Maier and Beach both spent time early on in Lashley's laboratory at the University of Chicago, before moving to the University of Michigan and the American Museum of Natural History (AMNH), respectively (Beach followed

Lashley to Harvard to complete his graduate training). A student of Stone's, Harlow spent his entire graduate career at Stanford, before moving to Wisconsin in 1930, where he remained until retiring as George Cary Comstock Research Professor of Psychology in 1974. Thus, only five institutions were examined in this section: Stanford, Michigan, Indiana, Wisconsin, and the American Museum of Natural History.

Referring to Figure 4 and Table 4, one can see that Wisconsin (d=21, wd-63) was the only institution that was top six in both degree and weighted degree. Stanford (d=10, wd=53) was just left off the list, but remained one of the most productive institutions on the list. Michigan (d=8, wd=35), Indiana (d=8, wd=17) and the Museum (d=5, wd=14) were less productive than Wisconsin and Stanford, but were still above the 80th percentile of institutions in terms of output in the *JCP*. All five of these institutions were located closest to the model organism of their incumbent psychologists: Stanford, Michigan, and the Museum were closest to the rat, Wisconsin was right beside the monkey, and Indiana was located nearby the dog. Maier, Kellogg, Harlow, and Beach did not join their respective institutions until after 1930. Thus, I once again viewed the network in two separate time periods.

Institutions in the Early Years (1911-1930)

The institution network from 1911 to 1930 (Figure 5) was consulted to determine Stone's influence in this period. Stone joined the Stanford faculty in the autumn of 1922 and immediately made his mark. The Stanford psychology department had been founded in 1892, when Frank Angell, a product of Wundt's lab in Leipzig, established the laboratory and assumed the position of Professor and Director of the Philosophy Department. From 1892 until he retired in 1921, only one article was published in the *JCP* from Stanford; an article by James Rollin Slonaker (1912) on the development of an albino rat. Angell, who was a year removed from establishing

the psychology laboratory at Cornell University, only trained one PhD student during his tenure at Stanford: John Edgar Coover. Succeeding Angell in 1922 was Lewis M. Terman, a graduate of G. Stanley Hall's Clark University. He was moved from a professorship in education to head the newly formed psychology department (Hastorf, 2004). Terman brought Stone in to serve as the resident comparative psychologist, joining a small but prominent faculty of Truman Kelley, prominent statistician influential in the development of factor analysis; Walter R. Miles, an experimental psychologist with many interests; Edward K. Strong Jr., the well known vocational psychologist; and Paul R. Farnsworth, who later became a social psychologist. The arrival of Stone invigorated the comparative program at Stanford, as it published sixteen articles on five species from 1923 to 1930. Of these sixteen articles, fourteen can be in some way connected to Stone either by first authorship, second authorship, or as an acknowledgment by the writer in a footnote. The Stanford comparative research program became even more productive post 1930. Stone's research acumen was assured by his supervisor, Lashley, to Terman in 1922: "In research he has shown a good bit of originality, and a very great perseverance showing unusual independence. His interest is chiefly in nervous and glandular mechanisms. He gives every indication that he will continue to be active in research" (cited in Rosvold, 1955, pp. 326-327). Lashley did not falsely advertise his pupil.

Maier, Kellogg, Harlow, and Beach did not publish under the affiliation of their home institution until after 1930, and thus, their influence could not be gauged in this network. However, looking at the degrees and weighted degrees of Michigan (d=5, wd=3), Wisconsin (d=11, wd=11), Indiana (d=2, wd=3), and the AMNH (d=0, wd=0) in this period allowed us to determine the institution's level of productivity prior to their incumbents arrival. Michigan and Indiana researchers were barely active in this period, publishing only three articles each on five

and two species, respectively. The Wisconsin laboratory, although only publishing eleven articles, was highly comparative studying eleven distinct organisms. And the AMNH did not publish in this time period.

Institutions in the Later Years (1931-1950)

Figure 6 shows the network from 1931 to 1950. Stanford's comparative research program continued to thrive in this period, publishing 36 articles on eight subjects. Stanford's model organism was, like many other institutions, the rat: 36 of the institution's 53 articles were on the rat. Stone was once again the dominant influence in Stanford's participation in the *JCP*, directly involved in 31 of the 36 articles (thirteen as first author, eleven as second author, and seven times acknowledged in a footnote). Stone dominated the comparative psychology program at Stanford in a way that no other laboratory head did.[26] Of course, much of the success of Stone's program was in large part due to the National Research Council's establishment of the Committee for the Research on Problems of Sex (CRPS), and the Rockefeller funding that sustained it. Stone, along with many of his students and collaborators, funded their research with CRPS grant money. Stone also became editor of the *JCP* in 1947 (when the name changed to *Journal of Comparative and Physiological Psychology*), but this did not seem to change his or Stanford's publication patterns.

Prior to the 1930s, the Michigan laboratory was barely active, publishing only three articles on five organisms. In fact, there was no publication in the *JCP* under the Michigan affiliation from 1914 to 1928. This period of inactivity ended in the 1930s with the arrival of Norman Maier. During this period, the department was under the headship of John F. Shepard (Thurma, 1932). However, the most active Michigan researcher contributing to the *JCP* was Maier, publishing 24 of the institution's 30 articles. Maier, who took his BA and doctorate at

[26] This, of course, refers only to the research output in the *JCP*.

Michigan under Shepard's supervision, returned to the university in 1931 and immediately boosted the department's research output. Indeed, Maier was singly responsible for 80 percent of Michigan's appearance in the journal, and without him, the department would have been stagnant. Further purporting Maier's influence at the institutional level, Michigan researchers were conducting studies on only the human and the rat, the same two organisms that Maier favored. The pattern of how often these organisms were studied also mimicked Maier's research program, with 26 studies on the rat and four on the human.

Similar to Michigan, Indiana was essentially inactive before the arrival of Kellogg in 1929. The institution's twelve publications in this later period on seven organisms were, in effect, a reflection of Kellogg's individual research. An article by Alfred F. Lindesmith (1946) on chimpanzees was the only Indiana affiliated paper that was not connected to Kellogg. Outside of his ten first-authored publications, he co-authored an article on the dog with E.L. Walker. With this information in hand, it goes without saying that the dog was the model organism for the Indiana laboratory. This was due in large part to Kellogg, but also due to the dog conditioning laboratory that was established in 1936. The relationship between Kellogg and Indiana provided an interesting scenario that was distinct from the others. Indiana's research output depended on Kellogg's research and without Kellogg on the faculty at Indiana, the institution's appearance in the network would have been meager. Although this can be seen as the dominant influence of Kellogg, it is more a byproduct of his role at Indiana. Benjamin and Bruce (1982) have argued that Kellogg was a "loner" at Indiana, and was not afforded the autonomous role as leader of the laboratory he desired. This led to him having only a few doctoral students at Indiana, and a rather paltry research program; especially when compared to the programs at Wisconsin and Stanford during the same period. The unappealing position he

found himself in was the reason he took less money to go to Florida State University in 1950. There, he abandoned the dog for the porpoise, and was given the leadership role he long desired.

Wisconsin before Harlow was highly comparative, studying insects such as mosquitoes, flies, butterflies, crickets, and beetles, the amphibian salamander, the arachnid scorpion, and water creatures such as fish, minnows, plankton, and sea urchin. This pre-Harlow comparative laboratory, under the purview of Joseph Jastrow (Cadwallader, 1987) was destroyed by the time Harlow arrived in 1930 (Rumbaugh, 1997). Upon his arrival, Harlow began to reshape the Wisconsin comparative research program into predominantly a primate research center. Indeed, in 1932, Harlow, with his own money and the assistance of his students, converted a house into a primate laboratory. Another account claimed that he had a primate laboratory put up on campus without the permission of the university; a move that almost cost him his job. But it was in 1953, with the support of the university, that he established his primate facility in a former cheese factory (Rumbaugh, 1997). This primate facility held important implications in the history of psychology. In this period, Wisconsin conducted 37 articles on ten species. None of the earlier species studied at Wisconsin were studied under Harlow's headship. Wisconsin undeniably became a center for primate research, conducting experiments on lemurs, orangutans, baboons, apes, gorillas, chimpanzees, humans, cats, dogs, and of course, monkeys. The only organisms of these ten that Harlow did not study were gorillas, chimpanzees, and dogs. The Wisconsin model organism was the monkey by a wide margin (26 of 37 articles published used monkeys). Harlow's influence went beyond this focus on primates, especially the monkey; like Stone, he contributed to 32 of the 37 articles published either through first authorship, second authorship, or in a supervisory role. An interesting finding was that Wisconsin did not, at any time, study the rat; one of the few institutions who could make such a claim.

The AMNH (d=5, wd=14) in this period was the least active of the five institutions. However, this characterization is misleading, as it takes into account only the publications in the *JCP*. The Museum was actually one of the most comparative institutions studying a diversity of organisms and a wide range of behaviors (Pettit et al., forthcoming/2015). This was due in large part to the intersection of two distinct disciplinary communities: G. Kingsley Noble's zoology and Frank Beach's neuropsychology which focused on sexual behavior during his time at the Museum. Noble's zoological program centered mostly on courtship behavior in a diversity of organisms. Pettit et al. (forthcoming/2015) argued that this exposure to the multispecies biology of the Museum influenced a change in Beach, ultimately leading to his condemnation of comparative psychology as the study of rat learning. This influence was not unidirectional, however, as Beach brought with him to the AMNH a greater emphasis on experimental control and quantification (Pettit et al., forthcoming/2015). Beach's first place of permanent employment was the Department of Biology at the AMNH in 1938. In terms of the Museum's participation in the *JCP*, Beach was the most active, contributing eleven of the total fourteen articles published in this journal. Moreover, Beach was the only researcher at the AMNH experimenting and publishing on the rat in the *JCP*. The other three articles focused on the cat, snake, turtle, and hamster. Upon the death of G. Kinsley Noble, the department chairman, lobbying by Beach, Yerkes, and Lashley saved the department from extinction, turning it into the Department of Animal Behavior – a place many prominent comparative psychologists would make their home. The AMNH, thus, developed a distinct culture of comparative psychology (Pettit et al., forthcoming/2015).

It is clear that all five of these authors were highly visible and influential in their respective institutions. Although each laboratory had their own distinct characteristics, the five

comparative psychobiologists shared two similarities: each author's program of research was the reason for the majority of their institution's publications; and the model organisms of Stone, Maier, Kellogg, Harlow, and Beach became the model organism of their institutional home. This institutional influence, coupled with their prolific individual contributions to the *JCP*, articulates Dewsbury's claim that a handful of "new generation" prominent psychologists helped the comparative discipline prosper in the 1930s and 1940s. In this case, these prominent psychologists conducted their research in the psychobiology outlined by Yerkes and Dunlap, and were integral in continuing in the comparative tradition.

Chapter Three (1920-1950): Neo-behaviorism and the *Journal of Comparative Psychology*

By the time of the controversial affair with Rosalie Rayner that led to his dismissal from Johns Hopkins in 1920, Watson found himself on the outside of academic circles, with his version of behaviorism being dropped by some and reformulated by others. Indeed, the period of 1920 to 1950 was dominated by the number of competing learning theories on the "lawfulness of behavior," that have collectively been termed neo-behaviorism. Like the psychobiologists, neo-behaviorists were interested in the study of behavior. However, rather than looking at the biological processes involved, the neo-behaviorists – like their predecessors Thorndike, Pavlov, and Watson –believed that the key to a scientific psychology was the study of learning and motivation through rigorously objective observational methods. Unlike their predecessors, the neo-behaviorists were much more interested in theory, and focused their efforts in trying to formalize laws of behavior (Amsel, 1989; O'Donnell, 1985). They were also influenced by philosophers Rudolph Carnap (1891–1970), Otto Neurath (1882–1945), and Herbert Feigl (1902–1988), who were leaders in the Vienna Circle of logical positivists. The logical positivists argued that science and meaningful knowledge can only be produced by physical observations of the world; anything else was either analytic or metaphysical nonsense. Thus, any statement made about a phenomenon under study must be constructed as a statement derived from physical observations (Smith, 1986). This philosophy further separated behaviorism from psychobiology because it removed speculations on "internal" mental operations from their repertoire. Whereas psychobiologists viewed behavior in organisms in its entirety (biological, environmental, physical, and even mental), neo-behaviorists confined themselves centrally to maze learning in white rats (later, conditioning in pigeons), and refused to accept concepts such as thinking, reasoning, and unobservable emotions in their vocabulary.

As the previous chapter assessed Dewsbury's claims on the comparative discipline, this chapter assesses those made by Schneirla and Beach. Schneirla first drew the attention of the disciplines practitioners to this overreliance on the rat, but Beach elaborated on it, boldly claiming that comparative psychology had disappeared and become behaviorism or the study of "rat learning." To investigate this claim using the network visualization, I shifted the focus toward the rat, the de facto model organism of the early neo-behaviorists (Benjamin, 2007; Dewsbury, 1984; Benjafield, 2005). In particular, I sought to examine the pattern in which the rat rose to become the most widely used species in the entire discipline. Moreover, since neo-behaviorism benefits from a rich historiography – many history of psychology textbooks dedicate significant portions to this part of the discipline's history (see Benjamin, 2007; Benjafield, 2005) – determining other hallmarks of the movement was simple. Like Yerkes, Dunlap, and Lashley were the "giants" in their psychobiology, Edward C. Tolman, Clark L. Hull, B.F. Skinner, and Edwin R. Guthrie were the four commonly recognized "giants" in neo-behaviorism (see Benjamin, 2007; O'Donnell, 1985; DeGrandpré & Buskist, 2000; Benjafield, 2005). Thus, I sought to determine their appearance in the network, looking specifically at their weighted degree and overall position (degree was not as important since I did not expect these predominately rat researchers to have been studying multiple organisms).

Unlike the psychobiologists, who published frequently in the *JCP*, these neo-behaviorists published more in general periodicals like the *Journal of Experimental Psychology*, *Psychological Review*, *Psychological Bulletin*, and *The American Journal of Psychology*. Thus, only Tolman and Hull appeared in the network (Skinner only published articles on laboratory equipment in this period). Since the most prominent neo-behaviorists were not the most prolific rat researchers in the network, I sought to determine the individuals who were the most closely

connected to the rodent. To accomplish this, I removed from the network any author who did not publish at least ten articles on a single species in order to elucidate the most prominent rat researchers. I paid no attention to the number of organisms studied (degree) in this step as the majority of neo-behaviorists studied only the rat and unlike their psychobiological counterparts, did not employ a comparative approach to "find" their model organism. Instead, the mounting studies conducted on the rat only further cemented its place in learning theory (Logan, 2005), and young researchers inherited the animal when entering a behaviorist laboratory.

Only eight authors remained after the previous step: Maier, Stone, Beach, Karl F. Muenzinger, Marion E. Bunch, Robert C. Tryon, Paul T. Young, and Paul S. Siegel. All eight researched the rat more than ten times. Maier, Stone, and Beach, characterized as psychobiologists and discussed in the previous chapter, were omitted from the list. With the remaining five authors, I wrote a brief biography of their academic careers, and examined their weighted degree and position within the network. Of course, with such a close connection to the rat, I expected that they would be positioned next to its cluster within the network. I argue that if these prominent rat researchers submitted to a behavioristic framework, then neo-behaviorism, through its connection to the rat, was influential in the journal. This would justify the part of Beach's claim that behaviorism was a rising force within comparative psychology.

The third and final part examines the institutions most closely connected to the rat. To uncover these institutions, I removed all institutions that studied a single organism less than thirty times. Thus, only the institutions that produced thirty articles on one organism were included. Only six institutions remained: the Yale Laboratories of Primate Biology, the University of California, the University of Chicago, Yale University, the University of Minnesota, and Stanford University. The YLPB was eliminated from the list because the favored

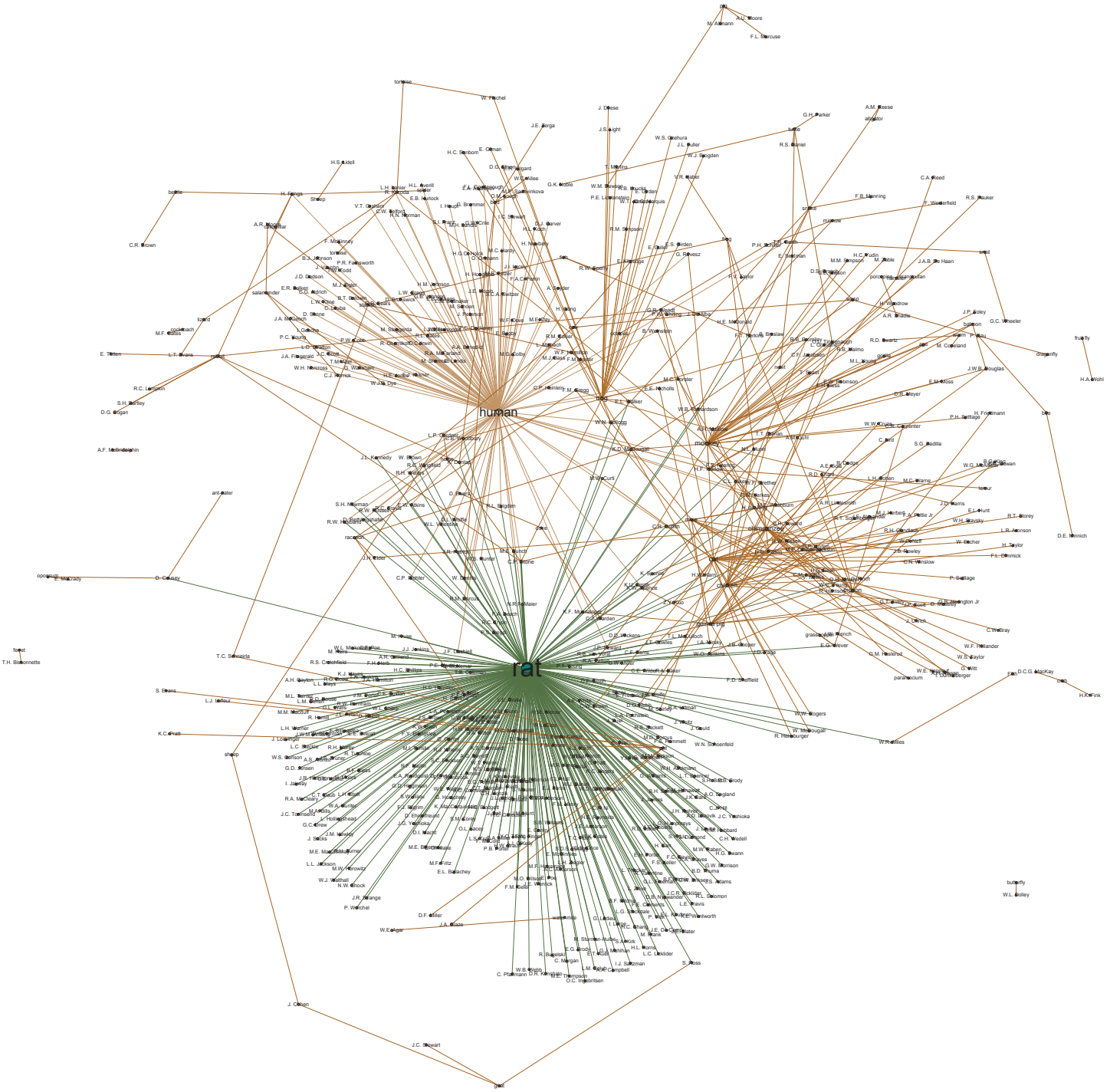
organism was the chimpanzee, and also because it featured in a previous chapter. Stanford's connection to the rat, discussed in the previous chapter, was directly linked to Calvin Stone's leadership of the comparative laboratory, and was thus omitted from the list. The four institutions that remained all favored the rat by conducting more than thirty articles on the species. This close connection indicated the possible presence of behavioristic theory; however, to determine if it was indeed present, I further examined the institution's participation (degree, weighted degree) in the journal, and members of the department's faculty. I argue that the institutions that employed the least comparative approach (lowest degree) of these prolific rat research centers were most likely to have submitted to a behavioristic approach. I also purport that the institutions that housed the most prominent behaviorists utilized the *JCP* as an outlet for their students' dissertations, and that these dissertations were behavioristically oriented (mostly on learning in the rat). This would further prove that Beach was partially correct in pointing out behaviorism's (in the form of rat learning) impact on the comparative discipline.

In the previous chapters, I looked at the period diachronically since there were two generations of psychobiologists. In this chapter, I looked at the period of 1920 to 1950 synchronically since all the main learning theorists proposed some form of their theoretical framework by the 1930s. Like previous chapters, I looked at two different network structures: an author-organism dyad; and an institution-organism dyad. With the current construction of my argument, I intend to make the case that Beach was correct in claiming that the influence of neo-behaviorism was vast in the *JCP* (although not to the extreme point of replacing the discipline), and was mostly witnessed through the rise of rat research. I argue that although many psychobiologists were studying the rat in equal or greater numbers, the rise of the rat's usage in the 1930s was directly connected to the number of neo-behavioristic theories that surfaced in the

decade. Furthermore, I purport that many of the most prolific rat researchers in this journal were affiliated with the neo-behavioristic scaffold. Finally, I illustrate that although the key neo-behaviorists were not the most active participants in the *JCP*, their institutions were among the most prolific rat research centers in the period. I contend that the "factory" approach of these institutions – that is to say, many "one-and-done" publications and less prolific authors – were indicative of the strong behavioristic research programs built there, and the reality that many young scholars inherited the rat as a research subject instead of "finding" their own model.

The Rat and the Neo-Behaviorist "Giants"

The first network I looked at displayed the relationship between author and organism from 1920 to 1950 (Figure 10) and was oriented based on weighted degree. In other words, the emphasis was placed on the number of studies conducted. This network consisted of 654 nodes and 744 edges. The second network displayed the connections between institutions and organisms for the same period, effectively replacing the authors from the dyad (Figure 11). Since there were more researchers than institutions, there was less data in this network: 238 nodes and 403 edges. The nodes in this case represented organisms and institutions. In many ways, the most salient aspect of these networks, the rat, was perhaps the most unsurprising. Many historians have written about the neo-behaviorists' (and non neo-behaviorists, for that matter) preference of the rat as an object to study learning (Dewsbury, 1984; Beach, 1950; Benjamin, Jr., 2007; Logan, 2005). Thus, the fact that the rat featured so prominently in both networks was not a revelation. However, the rat represented a gateway into the influence of neo-behaviorism in the *JCP*, and was therefore analyzed further.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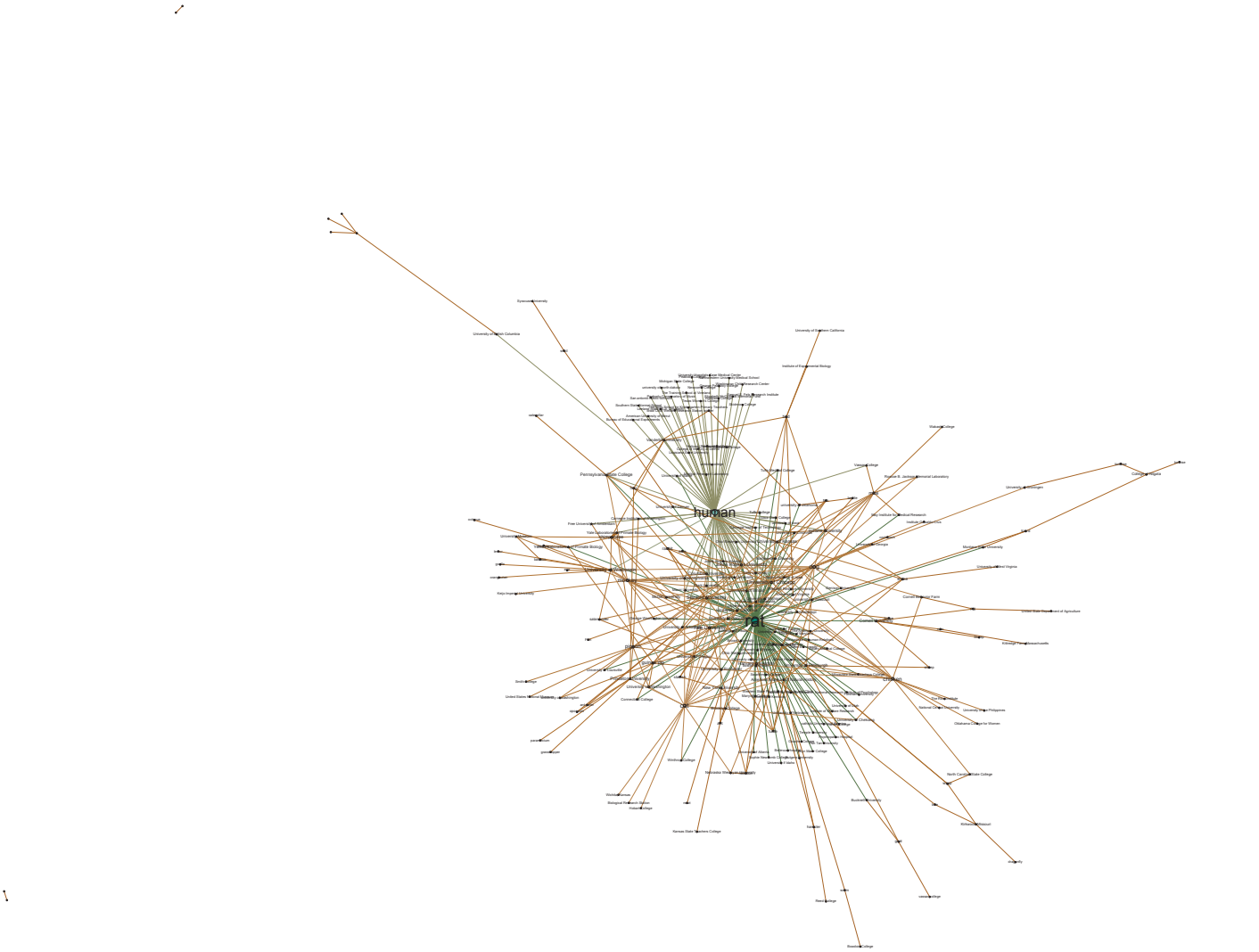

human
rat

Rattus Norvegicus

Viewing Figure 10, one can see that rats were the subject of more studies than any other organism in the *JCP* at the time. The rat (344) was studied by nearly three times as many researchers than its nearest counterpart, the human (121). Since two organisms cannot be connected, we know that 344 authors had published at least one study using the rat as the subject. Moving to Figure 11, it was apparent that more institutions were connected to the rat (99) than any other organism, including the human (74). Thus, in both networks, the rat possessed the highest degree, meaning more authors and institutions were studying the rat in the *JCP* than any other species. Moreover, the rat, with a weighted degree of 718, was studied more than four times as much as the human (175).

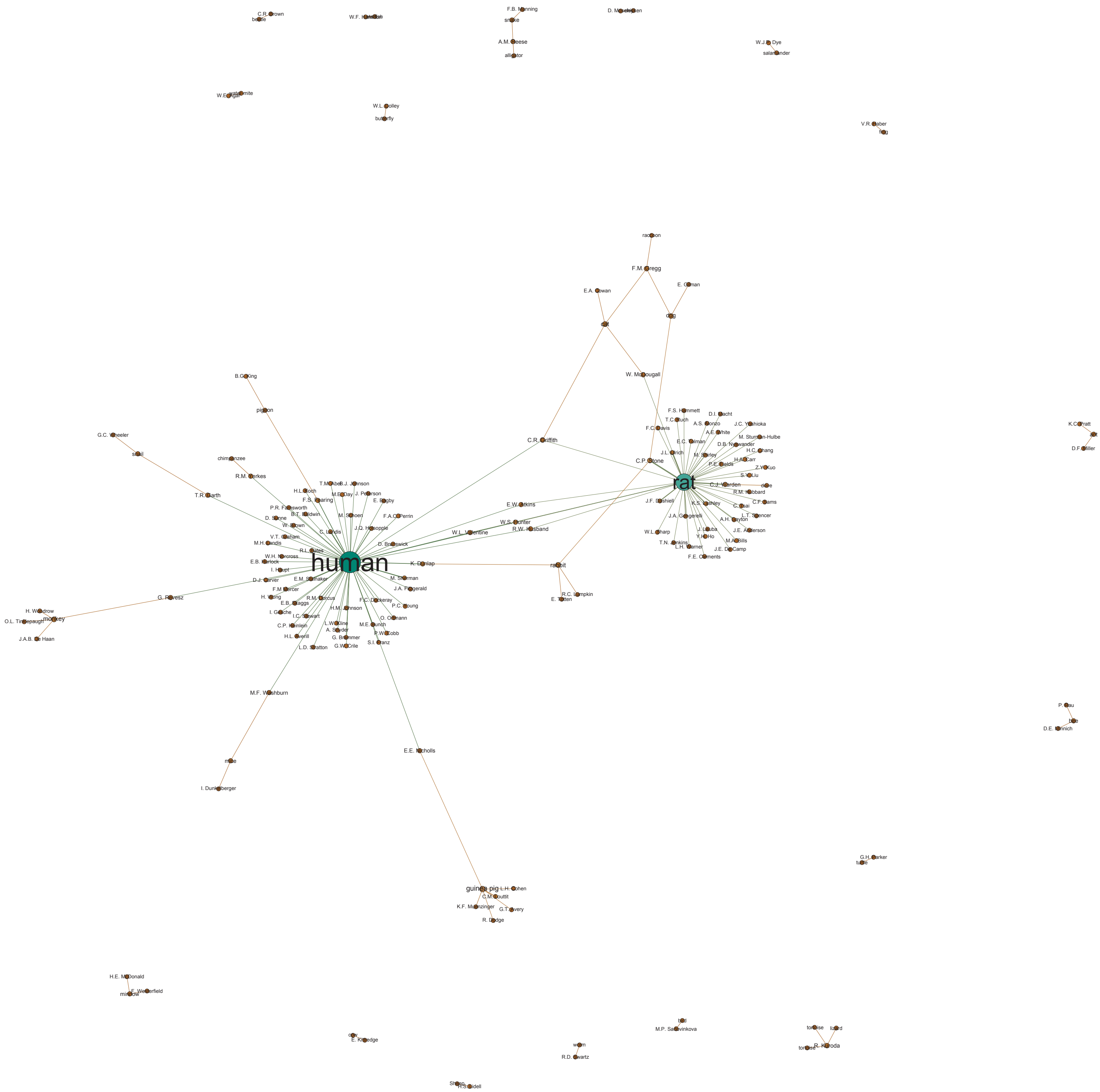
The visual and quantitative domination of the rat aligns historically with neo-behaviorism and learning theory. Some have argued that the use of the rat for research purposes was an accidental event (for example, Beach, 1950; Lockard, 1968). However, Dewsbury (1984) argued many of the earliest learning theorists emphasized selecting an organism in which studying learning would be appropriate (for example, Small, 1901; Miles, 1930). Thus, the rat was chosen specifically for its natural propensities and capacity with which to study learning. Furthermore, the relatively low cost and easy maintenance of colonies of rats contributed to the spread of the species as animal subjects. Thus, less prolific and smaller institutions were more likely to use the rat as a test subject due to cost effectiveness. As more researchers studied the rat, the bank of available knowledge on the animal grew, further cementing the use of the organism in studies on learning.[27] But the use of the rat was not confined to learning studies; the rat became a "standard organism" in related fields such as physiology and neurology, and also was a popular choice amongst psychologists studying development (Logan, 2005; Pettit et al., forthcoming/2015). The

[27] Skinner's pigeons also became a popular subject choice, but this did not occur until much later.

rat's atricial pattern of growth (slow and gradual development) made them amenable subjects to learning and developmental studies; psychologists were able to witness development unfold under controlled conditions (Logan, 2001). In 1906, Henry H. Donaldson began a very successful breeding program at the Wistar Institute which populated psychology and medical laboratories across North America (Pettit et al., forthcoming/2015). The problems many have identified with the spread of rats as an object of study generally stemmed from the researchers disregard for the natural propensities and limitations of the rat as a subject, leading to inappropriate generalizations. As Dewsbury (1984) explained, rats should not be classified as a good or bad research subject, but instead should be classified as suitable for some kinds of research, and unsuitable for other kinds.

The rat's prominence as depicted in the networks and historically is undisputed. However, this synchronic view of the journal did not account for the exponential growth of the rat after 1930 that was seen in the previous chapter. To help understand this growth and when it was greatest, I broke the network down into three periods covering three separate decades: 1920 to 1929; 1930 to 1939; and 1940 to 1949.

Figure 12 shows the network from 1920 to 1929. In this network, two large clusters formed; the largest cluster around the human, and the slightly smaller one around the rat. According to the network, the rat was only studied 69 times by 42 different researchers, whereas the human was studied 80 times by 57 distinct researchers. The network from 1930 to 1939 was depicted in Figure 13. A transformation occurred in this network: the rat cluster dramatically increased in size, dominating the network, while the human cluster decreased slightly. The rat was studied nearly 4.5 times more in this decade than the last, with 306 articles published. Moreover, 153 researchers conducted experiments on the rat in this decade, 3.5 times more than


human
rat
guinea pig
rabbit
monkey
pigeon
C.R. Griffith
C.P. Stone
W. McDougall
F.M. Gregg
E.W. Atkins
W.S. Hunter
R.W. Husband
K. Dunlap
G. Revesz
M.F. Washburn
E.E. Nicholls
A.M. Reese
alligator
R.M. Yerkes
T.R. Garth
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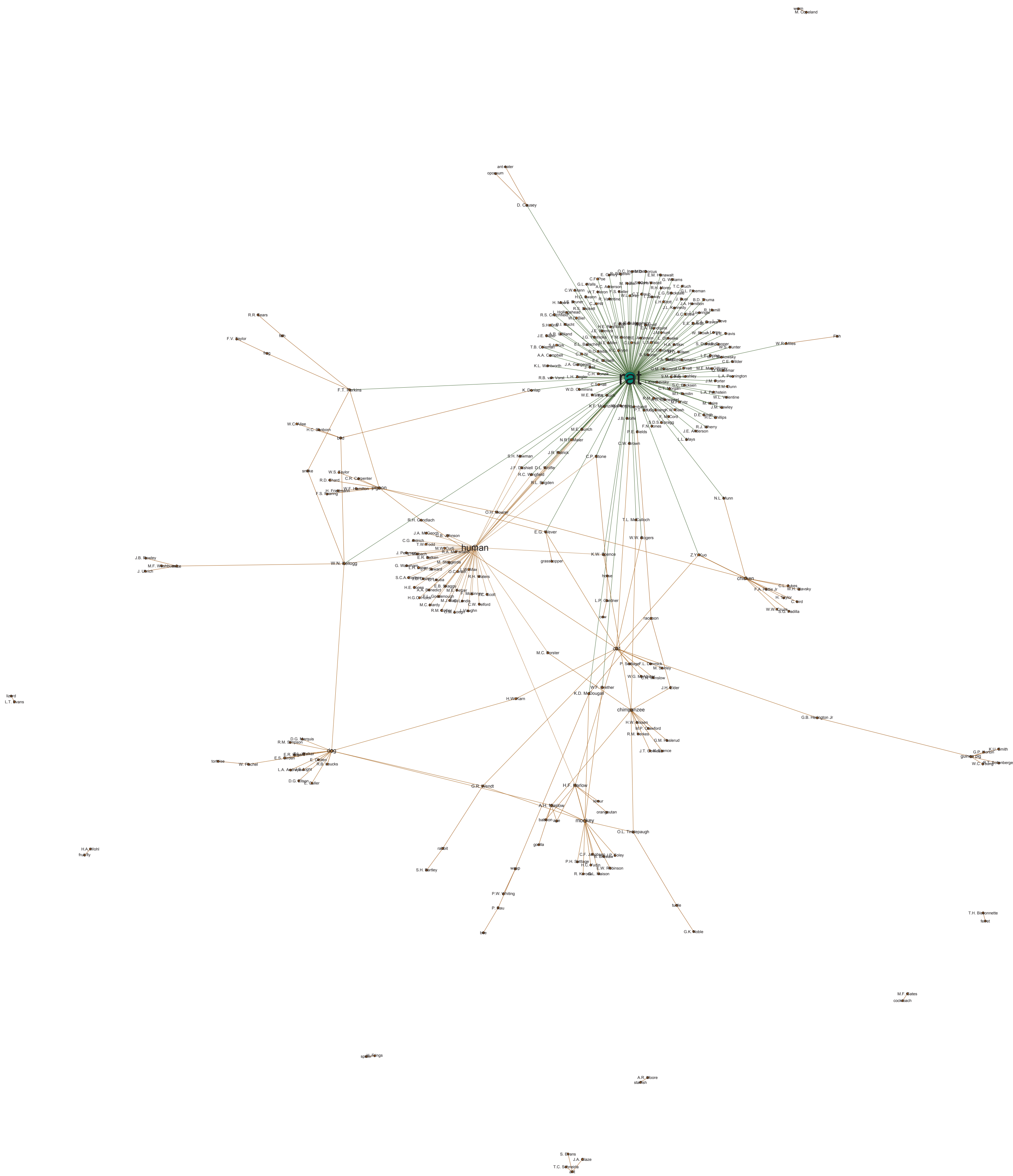
rat
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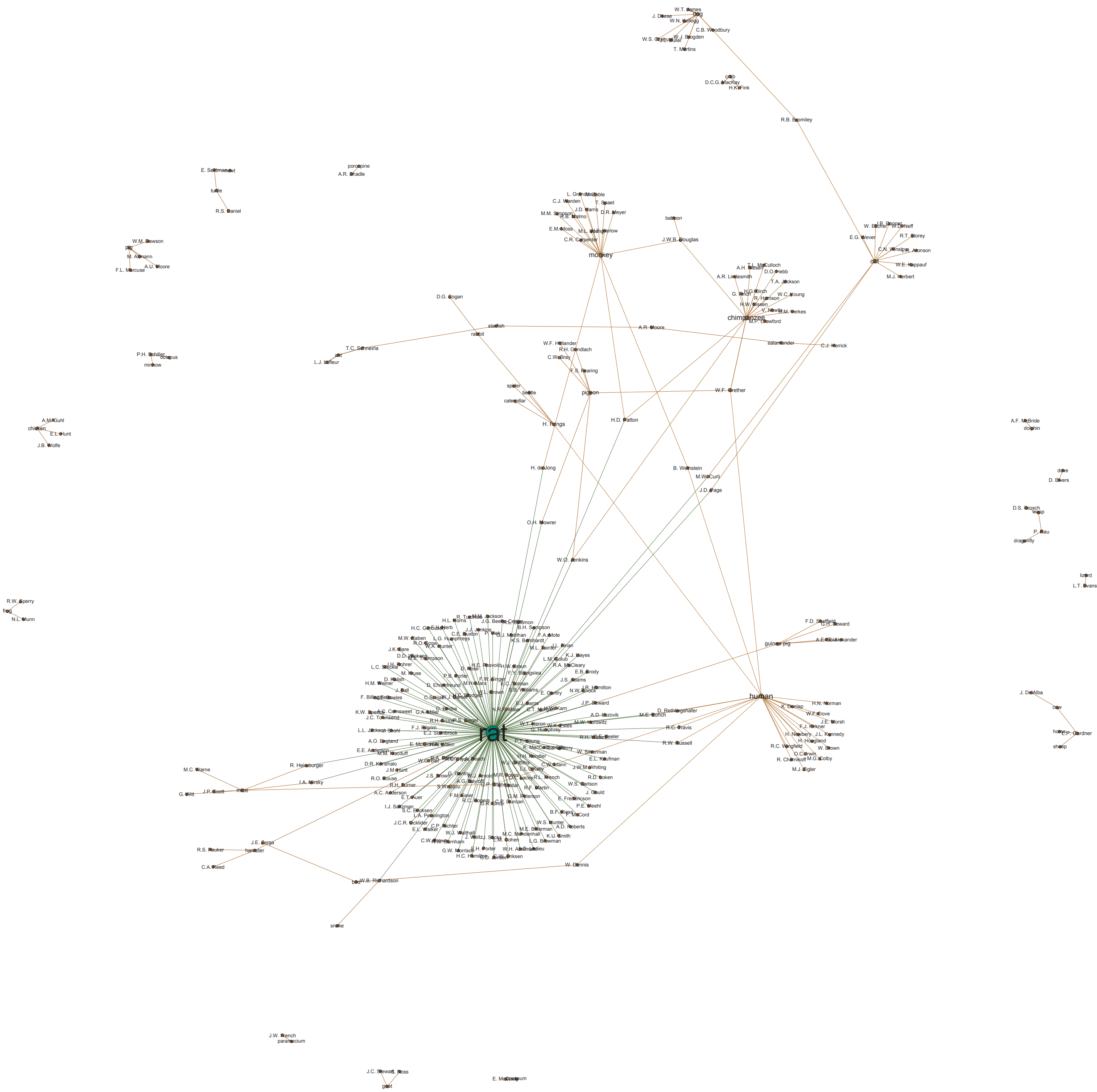

rat
human
monkey
chimpanzee
cat
pigeon
guinea pig
maze

the 1920s. Interestingly, the human could not sustain its growth from the previous decade, falling to only 64 studies by 48 researchers. Figure 14 displayed the final network covering the decade from 1940 to 1949. This decade resembled the previous decade in terms of the rat's use as a test subject. A total of 307 articles by 162 researchers essentially demonstrated that although the rat did not grow much, it maintained its popularity throughout this decade. The human, on the other hand, continued to drop, with only 26 studies by 23 researchers. The chimpanzee had even eclipsed the human in this decade in terms of output with 29 articles by 17 authors. Thus, it was apparent that from the 1930s on, the rat was the most popular test subject by a large margin. However, although the rat was studied more often and by more researchers in the 1940s, the species experienced its major growth in the 1930s. A plausible explanation for this growth might have been the emergence of the theoretical frameworks of the major neo-behaviorists in the 1930s.

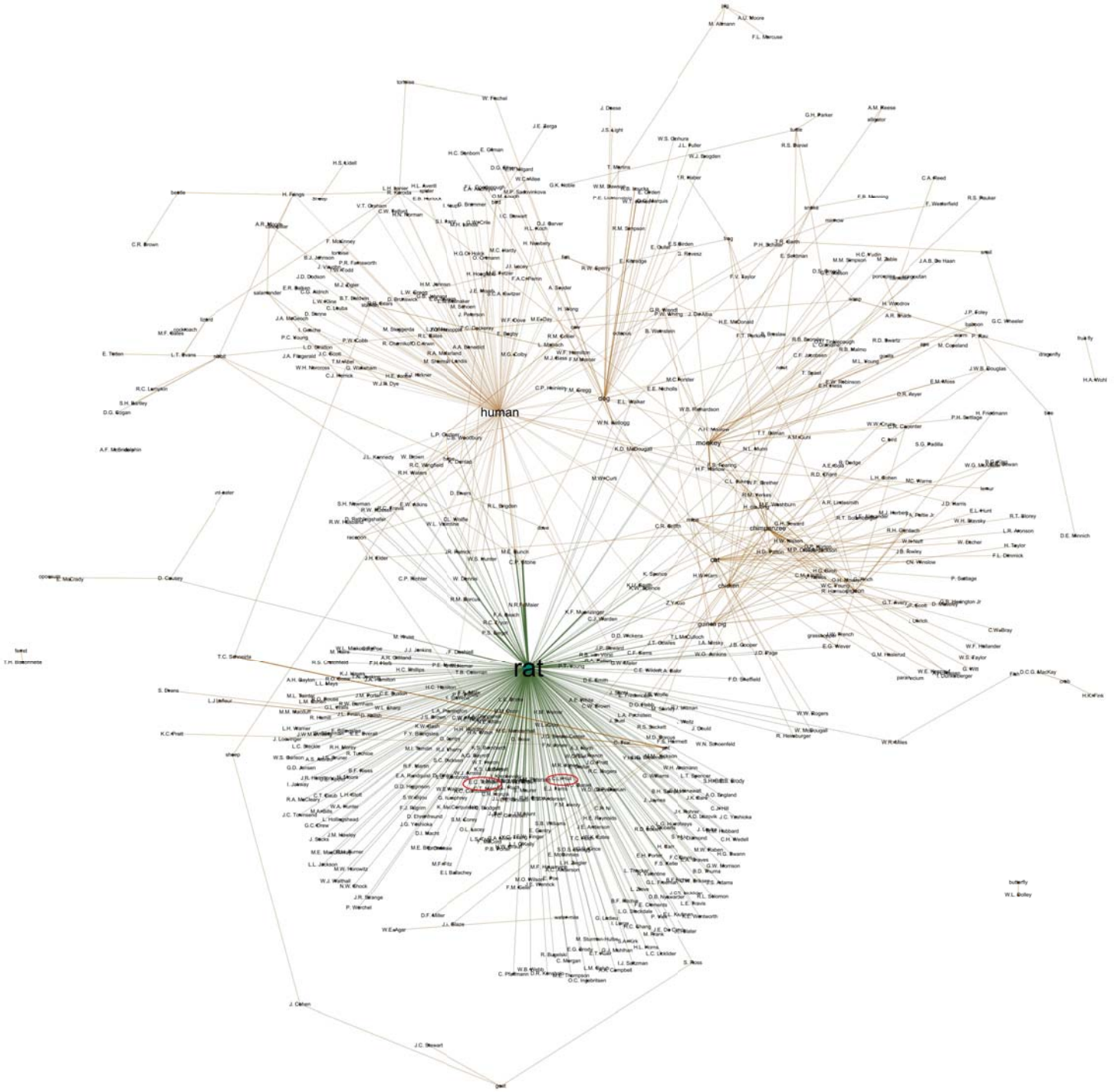
Tolman, Guthrie, Hull, and Skinner

All four of these behaviorists published some form of their theories in the 1930s: Tolman was the first, publishing his textbook *Purposive Behavior in Animals and Men* in 1932; Guthrie and Hull both published on their theories in 1935, Guthrie with his book entitled *The Psychology of Learning* and Hull's article in *Psychological Review* "The Conflicting Psychologies of Learning: A Way Out"; and Skinner was the last of the bunch, his textbook, *The behavior of organisms: An experimental analysis*, printed in 1938. Each one of these theorists or "system-builders" (Krech, 1959) also favored the rat as their test subjects; however, Skinner eventually moved from the rat to the pigeon (Rutherford, 2009; Benjamin, 2007). Although all were historically prolific, only Tolman and Hull appeared in the network;[28] albeit not as often as one might expect. This was an expected finding. Guthrie, although publishing extensively in his early

[28] Skinner published articles describing apparatus, and thus, did not qualify as a substantive article.

years as a philosopher, was much less prolific as a behaviorist where he only published three texts on human behavior (Clarke, 2005). Skinner did not attain his level of publication prominence until the 1960s (Benjamin, 2007). Tolman and Hull were much more active individually, and at the laboratory level in this era.

Figure 15 depicts the author-organism network from 1920 to 1950 with Tolman and Hull circled. Both authors were located close to the rat node, but unlike their psychobiologist counterparts – whose comparative work closely matched the original goals of the *JCP* – the neo-behaviorists were inconspicuously nestled within a large group of rat researchers. Tolman was the more active of the two by a slight margin: Tolman published five first-author articles on the rat and Hull contributed four rat experiments. Their weighted degrees made them moderately active in this journal and their degrees of one aligned with what was to be expected. Alone, the paucity of this output might suggest that these individuals were not active contributors to the *JCP*. However, Tolman and Hull were actively publishing in a number of journals at the time – *Journal of Experimental Psychology, Psychological Review, Psychological Bulletin, The American Journal of Psychology,* the *Journal of Abnormal and Social Psychology*, the *Journal of Educational Psychology*, and the *Journal of Philosophy* – and the *JCP* was among the most frequent of their chosen outlets. In terms of influence to the comparative discipline, Tolman and Hull exerted most of their influence through their theoretical frameworks and the output of their psychological laboratories (discussed in a later section).


human
rat

Purposive Behaviorism and the Hypothetico-deductive Method

The purposive behaviorism of Tolman and hypothetico-deductive method of Hull shared some similarities. Both frameworks utilized the white rat as a test subject, emphasized the importance of the objective study of overt behaviors, and adhered to some form of a stimulus-response relationship. However, the similarities stopped there, as both men advocated opposing theories on behavior.

Edward Chace Tolman (1886-1959) received his doctorate at Harvard under Hugo Münsterberg and Robert Yerkes before moving to the University of California, Berkeley in 1918, to spend the rest of his career. Tolman argued for a molar or holistic approach towards the study of learning and behavior. For him, behavior was *purposive*; indeed, his most important textbook, published in 1932, was titled *Purposive Behavior in Animals and Men.* He believed that behavior did not just occur spontaneously, but rather, was directed towards a particular goal.

Further separating him from his many behaviorist brethren, Tolman believed that he had reduced cognitive concepts (purpose, mental maps, etc.) to behaviorial terms, and thus retained cognitive processes as the underlying mechanisms for this purposive behavior. Tolman could not understand how anyone who had witnessed rats in a maze could argue against the purposive nature of the rat's behavior. Tolman believed that rats had *cognitive maps* or spatial representations of their environment that led to effective functioning. He argued that as an animal (rat) attained experience of their environment, they built up expectancies, which in turn, acted as a determinant in how the animal would respond in the future. Tolman (1951) demonstrated his theory of cognitive maps by testing two groups of rats in a plus shaped maze. For the one group (response learners), the rats had to always turn right to receive food no matter which arm they started off in. The other group (place learners), were always fed in the same

place regardless of which arm they started off in. Thus, the response learners had to learn to turn right to be fed, and the place learners had to learn to go to the same part of the maze on each trial. Tolman found that the place learners learned the maze more quickly than the response learners, indicating the use of cognitive maps as a tool to guide the rats learning.

Tolman also objected to Watson's approach of a strict stimulus-response in understanding behavior. The Berkeley professor did not believe that psychology should be limited to such a simplistic framework, and called for the recognition of intervening variables that "intervened" between the stimulus and the response. Cognitive processes were Tolman's examples of an intervening variable, and so long as they were supported by observations, they could be scientifically respectable.

Tolman also questioned the role of reinforcement in learning. Hull and B.F. Skinner both believed that reinforcement was necessary for learning to occur. Tolman disagreed, citing his studies on latent learning as an example of a contradiction. In one particular study, Tolman (1948) placed a rat in a maze and did not present any food. Even without a reinforcer, Tolman argued that the rat still began to construct a cognitive map of the maze so much so that when placed in the maze with food present, the rat did better than an entirely naïve rat.

Clark Leonard Hull (1884-1952) earned his doctorate in 1913 under the mentorship of Joseph Jastrow, Daniel Starch, and V.A.C. Henmon at the University of Wisconsin. He remained at Wisconsin until 1929, when he left his teaching post to join the prestigious Rockefeller funded Institute of Human Relations at Yale University. He held this new position for the remainder of life. Much of Hull's early work focused on aptitude testing and hypnosis, but it was his theory of behavior on which he became the most cited psychologist of his time (Benjamin, Jr., 2007).

Of all the neo-behaviorists, Hull was the most ambitious in formalizing laws of behavior, and he believed he had determined the fundamental laws of learning and habit formation that underlay the behavior of all humans and animals. Hull (1952) conceptualized his theory as follows:

> I came to the definite conclusion around 1930 that psychology is a true natural science; that its primary laws are expressible quantitatively by means of a moderate number of ordinary equations; that all the complex behavior of single individuals will ultimately be derivable as secondary laws from (1) these primary laws together with (2) the conditions under which the behavior occurs; and that all the behavior of groups as a whole, i.e., strictly social behavior as such, may similarly be derived as quantitative laws from the same primary equations (Hull, 1952, p. 155).

Hull's faith in the lawfulness of human behavior would eventually lead to the demise of his theory.

According to the fundamental law Hull believed guided behavior, an unconventional stimulus could bring about a response as long as it was associated, either temporally or in nature, with the stimulus that usually elicits the response. This effect had previously been noted by Pavlov when his dogs salivated at the unconventional stimulus (ringing bell) after it had been paired with a natural stimulus that elicits salivating (food). Similar to Pavlov's classical conditioning, Hull also theorized that learning was not all or nothing, but instead occurred incrementally. The animal gradually learned to respond to all the aspects of a positive stimulus or to avoid a negative stimulus, and thus, stimuli could be engineered to precisely control the forming of particular habits. Unlike Tolman, Hull's theory of behavior was mechanistic, and thus, removed any talk of consciousness, the soul, or free will. Hull, who at one point hoped to

be an engineer, designed working machines that demonstrated the principles of conditioning reflexes, further purporting the mechanistic functioning of human behavior. Moreover, as his own conceptualization of his theory suggests, Hull reduced behavior down to a series of complex mathematical equations, and his 1943 book *Principles of Behavior: An Introduction to Behavior Theory* was filled with them (Hull, 1943; Amsel, 1989; Amsel & Rashotte, 1984; Benjamin, Jr., 2007; O'Donnell, 1985).

To test the validity of his grand theory, Hull used what he called the *hypothetico-deductive method*. Inspired by the natural philosopher, Isaac Newton, and his scientific certainty of knowledge, Hull sought to establish his laws of behavior strictly on firm observational and experimental techniques. His method began by stating a postulate or series of postulates from which testable hypotheses could be deduced and tested experimentally, which in turn lead to either a confirmation of the hypothesis or a disconfirmation. Upon a disconfirmation, the postulate would be modified so that a new hypothesis is deduced from the revised postulate. This process continued until the hypothesis is experimentally and observationally proven.

Hull also differed from Tolman on his views of the importance of reinforcement in learning. Without reinforcement, Hull believed learning could not occur. At the crux of his theory of reinforcement was the idea of *drive reduction*. That is to say, certain bodily needs such as hunger, sex, thirst, oxygen, etc., would govern how an organism behaves; if the hunger drive is strong enough, the organism will seek out food to engage in feeding behavior to reduce that drive (Hull, 1943). This reduction in drive served as reinforcement, ensuring that the behavior is repeated in similar situations. Moreover, reinforcement was the key to the development of *habit strength*, a concept that Hull used to describe the association between a stimulus and a response. The higher the number of reinforcements of a particular association, the more the habit strength

would grow. Habit strength, essentially, was the main indicator of the strength of learning (Benjamin, Jr., 2007).

Tolman's theory of behavior was ahead of his time and thus, the psychological community responded better to Hull's theory. Historians believed this to be a byproduct of Tolman's use of cognition in a time when psychology was trying desperately to rid itself of any ties to consciousness. However, Tolman's laboratory remained one of the most productive rat research centers of its day that also contributed to the development of a behavioral genetics sub-discipline. Moreover, in the 1960s and 1970s, when psychologists were once again talking about consciousness, Tolman's ideas were rediscovered and found to be extremely important. Some of these ideas were the demonstration of latent learning, the concept of cognitive maps, the characteristics of response and place learners, and the distinction between performance and learning. For Tolman, performance was the observable behavior, and learning was the internal state. Thus, one could not directly observe learning, but could infer it based on the performance (behavior) of the animal (Benjamin, Jr., 2007; Dewsbury, 1984; Amsel, 1989; Tolman, 1922).

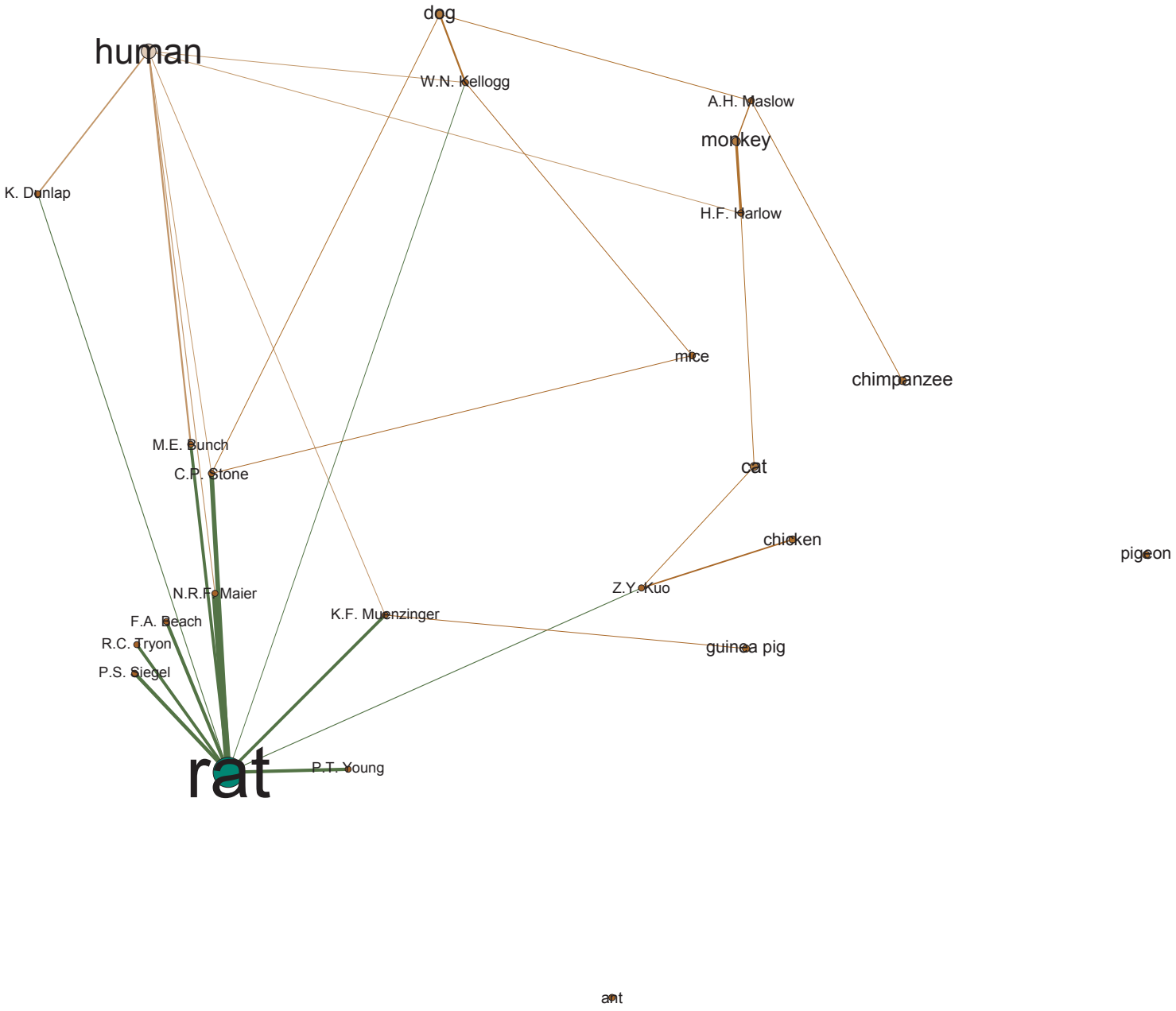
The very nature of Hull's program of research meant that he developed many diverse constructs; only a few of which were mentioned in this section. Hull created a psychology theory of behavior that has yet to be matched; one that allows researchers to generate testable hypotheses. His theory was most popular in the 1940s. And because he created "an industry in experimental psychology" (Benjamin, Jr., 2007, p. 148), two decades worth of doctoral students had no issues in finding testable hypotheses on which to base their research. Most of these dissertations were learning studies on the rat. The psychological literature was filled with Hullian theory, making him the most cited psychologist of his time: "during the decade 1941-1950 approximately 40 percent of all experimental articles published in the *Journal of Experimental*

Psychology and the *Journal of Comparative and Physiological Psychology* included references to his work" (Amsel & Rashotte, 1984, p. 2). Inevitably, Hull's belief in the lawfulness of behavior was his undoing. As Webster and Coleman (1992) have explained, "Hull's impact and subsequent decline [were] inevitable outcomes of the programmatic nature of [his] behavior theory" (p. 1063). Even though Hull's theory was eventually disproven, historians acknowledge his importance in establishing scientific rigor in psychology (Mills, 1998).

Tolman and Hull were two of the most influential figures in all of psychology's history. Individually, the network failed to depict the influence of these two neo-behaviorists. However, it seems likely that their frameworks were largely responsible for the rise of the rat as test subjects in the *JCP* in the 1930s and 1940s. Moreover, as will be shown in the latter portion of this chapter, the University of California and Yale's Institute of Human Relations were highly productive rat research centers under the headship of Tolman and Hull. Thus, unlike in the previous chapter, where the psychobiologists themselves were highly influential nodes, the influence of the major neo-behaviorists was seen through the rise of the rat as a test species, and the adoption of a neo-behavioristic framework in the *JCP*. To determine the influence of their theory on the comparative discipline, I examined the theoretical affiliation of the authors most closely connected to the rat.

"The Rat Pack"

The next step in determining the influence of behaviorism (as seen through the frameworks of the major neo-behaviorists) required eliminating from the network any individual who did not study the rat at least ten times to elucidate the most prolific rat researchers.


dog
human
W.N. Kellogg
A.H. Maslow
monkey
K. Dunlap
H.F. Harlow
mice
chimpanzee
M.E. Bunch
C.P. Stone
cat
chicken
pigeon
N.R.F. Maier
Z.Y. Kuo
F.A. Beach
K.F. Muenzinger
R.C. Tryon
guinea pig
P.S. Siegel
rat
P.T. Young
ant

As was mentioned earlier, eight individuals were left (Figure 16) with only five relevant to the chapter: Karl F. Muenzinger (d=3, wd=13),[29] Marion E. Bunch (d=2, wd=16), Paul S. Siegel (d=1, wd=12), Robert C. Tryon (d=1, wd=10), and Paul T. Young (d=1, wd=11). These five represented the most prominent rat researchers in terms of quantity of publications. Muenzinger, Bunch, and Siegel were considered "dust bowl neo-behaviorists" since each focused on empirical observation and data collection while adhering to a neo-behavioristic framework. Tryon was a student of Tolman's, and although somewhat of a "dust bowl neo-behaviorist" operating under Tolman's framework, his contribution to psychology went far beyond his association with the prominent neo-behaviorist. And Young was the only one that did not adhere to a behavioristic framework, but instead, was a Titchenerian Structuralist. Thus, it appeared that four of the most prominent rat researchers in the network were indeed influenced by the neo-behavioristic theories of the day, providing support for Beach's claim. Yet, several rat researchers, including Beach himself, did not adhere to neo-behaviorism.

"Dust Bowl Neo-Behaviorists"

The reason I characterized Muenzinger, Bunch, and Siegel as "dust bowl neo-behaviorists" instead of the more common "dust bowl empiricist" was twofold: similar to a "dust bowl empiricist," all three focused strictly on empirical observation with no attempt at building their own theoretical framework; however, in contrast to a "dust bowl empiricist," these scientists were not staunchly atheoretical, and instead, each operated under the neo-behavioristic umbrella. This was in stark contrast to Tolman and Hull who were two of the most recognized system-builders in the history of learning. Muenzinger aligned himself with Tolman's purposive behaviorism (Krech, 1959). Contrarily, Bunch and Siegel did not submit to any single

[29] (Degree, weighted degree)

framework, but rather conducted their research under the auspices of the greater neo-behaviorist umbrella (Vanderplas, 1997; Pate & Fowler, 2005).

Karl Friedrich Muenzinger attained his doctorate at the University of Chicago in 1918. In 1923, Muenzinger took up a faculty position at the University of Colorado in Boulder and by 1928, he was promoted to Associate Professor. It was in this year that Muenzinger began publishing in the *JCP*. Muenzinger was raised to the full rank of Professor in 1938, the year he ceased publishing in the *JCP*. Muenzinger (d=3, wd=13) published thirteen papers on three different organisms in the *JCP*: guinea pigs, humans, and rats. His first two articles (1928, 1929) were on habit formation in guinea pigs. He then moved on to study tone discrimination in rats (1931) and electric shock thresholds in humans (1932). The paper on humans was Muenzinger's attempt to standardize electric shock intensities so that they could be used on other animals, in his case, rats. The remainder of his articles in the *JCP* dealt with vicarious trial and error (VTE) discrimination learning in rats, which he preferred due to his interests in learning theory and systematic psychology.

Krech (1959) characterized Muenzinger's work as "science oriented" rather than "Muenzinger oriented"; his work was mostly recognized for its empirical findings, not how it contributed to building or sustaining a theory. Muenzinger's work aligned closely with Edward Tolman's theoretical framework, of whom he was a great admirer. Indeed, Muenzinger ascribed the theoretical backbone of his VTE studies to Tolman. Krech also cited his predisposition as another facet for why Muenzinger did not bother working out his own system. As a thorough researcher, Muenzinger was more concerned with answering his original research question through rigorous experiments and replications rather than conducting single superficial experiments to merely "make his point," a trait indicative of the hit-and-run style that

characterized the experimental work of many system builders (however exceptional the odd successful hit).

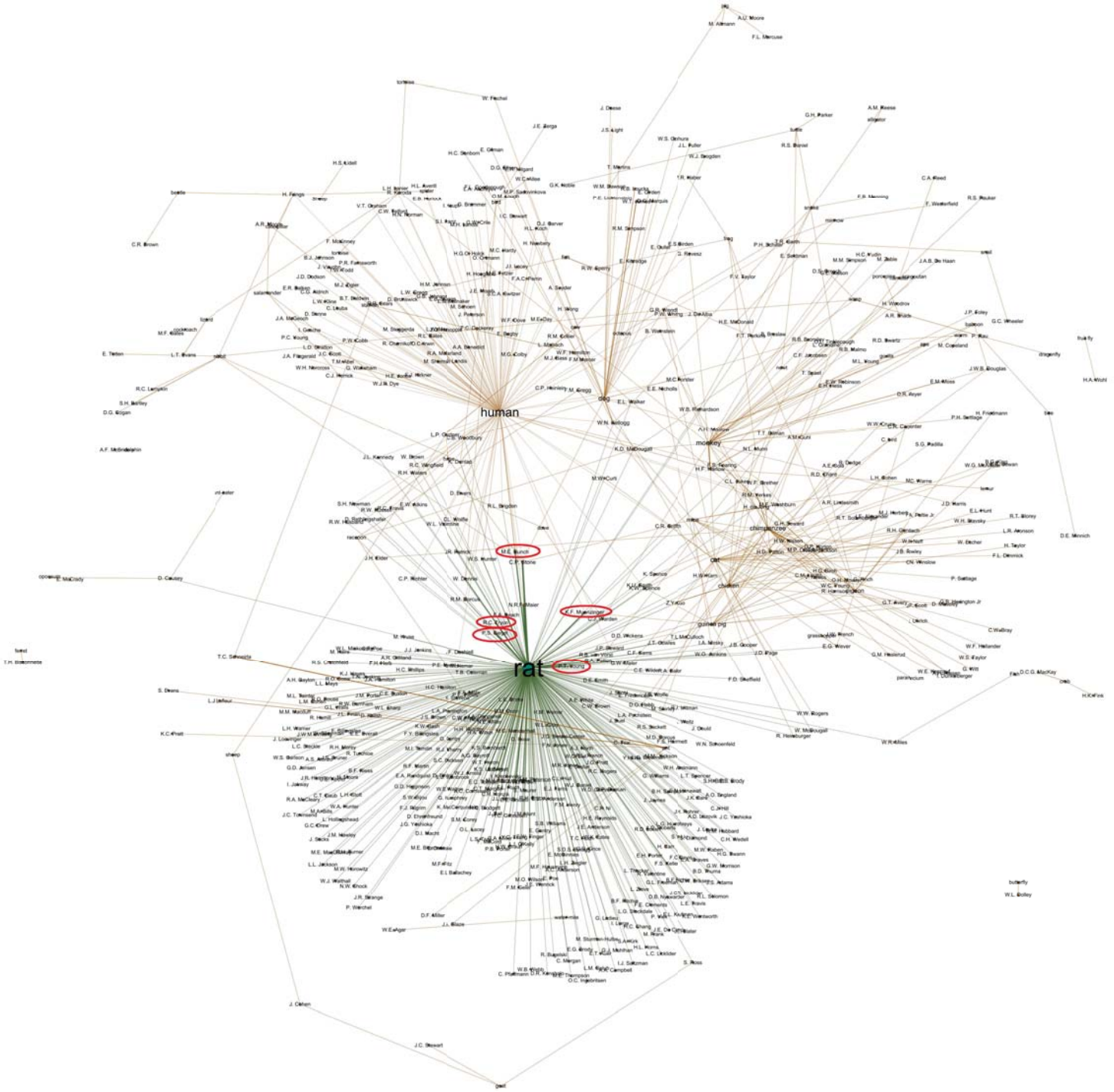
Marion E. Bunch undertook his undergraduate studies at the University of Kentucky, taking his BA degree in 1925. From there, he moved to St. Louis, Missouri, where he graduated with an MA degree at Washington University in 1926. Immediately thereafter, he was offered a position as an instructor by then department chair, John Whitely, at the urging of Bunch's supervisor, John McGeoch. He spent the next eight years teaching and conducting research at Washington University as an MA. His first publication in the *JCP*, in 1928, was a study on the effects of electric shock on maze learning in humans. However, Bunch was mostly concerned with learning, motivation, memory, and perception in rats, and how these processes contributed to the adaptive aspects of the organism. Bunch (d=2, wd=16) conducted studies on two organisms, humans and rats. Of the sixteen articles, eleven were on the rat, and five on the human. During his time at Washington University, Bunch spent his summers at the University of Chicago studying with Harvey Carr; a member of the functionalist school who supervised Bunch's doctoral degree in 1934. Bunch's work characterized the program of research he advocated which included learning, memory retention, transfer of training, perception, motivation, and later on, aging and gerontology. By 1949, Bunch was offered and took the position of chair of the psychology department at Washington University; a position he held for over twenty years. It appeared that although Bunch studied under the functionalist, Harvey Carr, he conducted his own program of research that fit under the behavioristic umbrella and, like Muenzinger, he did not seek to build his own theory (Vanderplas, 1997). Much of Bunch's later work came at a time when behaviorism relinquished its hold as the governing psychological

framework in learning and motivation; a time when psychologists were once again able to write and speak about consciousness.

Paul Shafer Siegel studied English as an undergraduate, taking his bachelor's degree in 1938 from the University of Richmond. Switching over from English to psychology, Siegel received his MA at Duke University in 1942. It was at Duke that Siegel found his two greatest influences, his classmates Sigmund Koch and Edward Stainbrook (Pate & Fowler, 2005). Since the Gestalt approach was dominant at Duke during the time Siegel was there, his early research on the Von Restorff effect[30] was based upon Gestalt principles. He then moved to the University of North Carolina where he received his PhD under Robert Wherry in 1944 (Pate & Fowler, 2005), before taking a professorship at the University of Alabama. What is interesting about Siegel's (d=1, wd=12) contributions to the *JCP* (during the period of 1920-1950), was that they all came between the years 1943 and 1950. All twelve of his articles were on the rat. Not only was Siegel one of the most active rat researchers of this time, he was perhaps the most active researcher in this eight year period.

Siegel's primary research interests laid in learning and motivation. Although he was influenced early by Gestalt theory, he quickly adapted his research to include the dominant behavioral theories. In fact, his first few articles on drive shift, the effect of electroshock on maze learning, habit strength, and extinction (Siegel, 1943; 1943; 1946; 1946; 1947; 1947), relied on the previous work of four established neo-behaviorists: Hull, Muenzinger, Skinner, and Tolman. He moved on to the investigation of audiogenic seizures, the effect of emotionality on food and water intake, and the importance of the dark-light cycle. As behaviorism's hold on learning theory began to wane in the 1950s (DeGrandpré & Buskist, 2000), Siegel left his research on rats

[30] The Von Restorff effect is named after psychiatrist and pediatrician, Hedwig von Restorff. Also termed the isolation effect, it asserts that something unusual is more likely to be remembered (Gumenik & Levitt, 1968).


human
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and began studying behavior in "retardates," investigating in these research participants discrimination learning and incentive motivation. Prolific though he was in terms of the quantity of his contributions, Siegel was mostly remembered as a mentor, teacher, and administrator (Pate & Fowler, 2005).

Muenzinger, Bunch, and Siegel were content to accept and work under the dominant disciplinary paradigm of the era, accepting the behavioristic emphases as scientific truth and assimilating their own research into this framework. However, although they were "dust bowl empiricists," all three were highly productive in the *JCP* and influential to the comparative discipline. Figure 17 depicts all five rat researchers circled in the network from 1920 to 1950. Looking particularly at Muenzinger, Bunch, and Siegel, all three were positioned closest to the rat in the least populated side of the cluster, which was in contrast to the positioning of Tolman and Hull within the mass of rat researchers. Their close connection to the rat also positioned them near the middle (or main part) of the network; the rat cluster was the most dominant so it made up the majority of the overall network. Muenzinger connected the rat, human, and guinea pig clusters, but was closest to the rat. Bunch was a mediator between the rat and human cluster. His five articles on the human pulled him further from the rat and closer to the human than the other two. Siegel, who was connected only to the rat, was the closest of the three to the dominant rat cluster. Thus, "dust bowl empiricists" or not, these neo-behaviorists were highly influential in the *JCP*.

Robert Choate Tryon

Tryon spent his entire career at the University of California at Berkeley; first as an undergraduate, receiving his BA in 1924; then as a graduate student, taking his doctorate under Tolman in 1928; then as a National Research Council fellow for two years; and finally as faculty

member of the psychology department in 1931. The only time he left Berkeley was for a short period in which he contributed to the war effort, serving as deputy chief of the planning staff of the Office of Strategic Services in Washington.

Tryon's contribution to psychology began as early as graduate school. Working alongside Tolman and Lloyd Jeffress, he developed an apparatus that automatically recorded animal behavior in a laboratory setting; arguably the first of its kind. His dissertation, "Individual Differences at Successive Stages of Learning," in 1928, was a landmark study in animal behavior in which he was able to measure individual differences in rat behavior with unparalleled degree of reliability. By melding statistical reliability and measurement with objective behavioral observation, Tryon succeeded in establishing a new precedent for future psychologists. He continued the line of inquiry established in his dissertation throughout his career.

In 1919, Tolman began a large-scale research project on inheritance in rats using a grant of $105 that he received from the Board of Research at the University of California. Specifically, he was granted the funding "for the study of inheritance of unusual ability in learning as exhibited by lower mammals." This extensive project was the first of its kind that bred distinct lineages of rats chosen specifically for their maze performances in order to determine the genetic component of maze learning. He presented his data from this project in his article "The Inheritance of Maze-Learning Ability in Rats" in 1924 in the *JCP*. Although this was groundbreaking research, it was clearly just the beginning of an ongoing research program.

The study began with 82 rats (the initial generation), each of which was subjected to a maze with four choice points. Their performance on the maze was measured by number of errors committed, perfect runs, and the time it took to complete a trial. Male and female rats that performed the best on the maze were mated as were the male and female rats that performed the

worse, starting a *maze-bright strain* and a *maze-dull strain*, respectively. The offspring of these rats (F_1 generation) were then mated in a similar fashion to that of their parents, leading to a third generation (F_2) of *maze-bright* and *maze-dull* rats. Tolman found that the F_1 generation of *maze-bright* rats performed better than the initial generation, but the F_2 generation did not perform as well at the F_1 rats. Furthermore, the F_2*maze-dull* rats performed about as poorly as the F_1generation. Tolman was unsure as to why this was the case, noting the age of testing, nutrition, inbreeding, and environmental conditions as possible reasons. In any case, Tolman adopted a philosophy of "learning from one's mistakes," and set out to construct a more rigorous approach to the research (Innis, 1992).

Tryon, who had enrolled as a graduate student in 1924, possessed unique skill and interest in genetics and statistics – two proficiencies that were invaluable to work on the inheritance project. Naturally, Tolman was eager to get him involved in the project. From 1924 till the time he defended his dissertation in 1928, Tryon, along with others, worked on the problems that arose from the original inheritance project.[31] By the time that Tryon initiated the decisive study in 1927 – a study which spanned over a decade and included more than 20 generations of maze-bright and dull rats – these problems had been rectified.[32]

By 1927, Tryon had essentially taken control of the inheritance project. Tryon continued this line of research for over a decade and more than 20 generations. However, he noted that no further divergence of populations occurred past the eighth generation (see Tryon, 1940; 1942). Tryon's importance to the success of the inheritance project cannot be understated. In fact, some

[31] The problems were outlined succinctly by Innis as "(a) the reliability and validity of the measures of maze learning, which entailed consideration of both the adequacy of statistical treatments and the generality of the findings from a particular maze; (b) the nature of the initial subject population and the method of selecting mates in future generations; and (c) the control of the environmental variables, such as living conditions, diet, and handling" (Innis, 1992, p. 193).

[32] For a more detailed account of this historical event and how each problem was solved, see Innis, 1992.

have credited him as the pioneer of behavior genetics, noting that the inheritance study he undertook was the first successful study on the genetic basis of maze learning, which established the sub-discipline of experimental behavior genetics (Krech, Crutchfield, &Ghiselli, 1969). His importance to the inheritance project was most aptly displayed when Tolman had leveraged his own offer from Harvard (something he had desired as a young professor at Berkeley) to obtain an assistant professorship for Tryon at California in 1931 – a position he held until his death in 1967 (Innis, 1992).

It goes without saying that Tryon (d=1, wd=10) was a neo-behaviorist, or, at least a Tolmanian neo-behaviorist, and thus, his work was strongly influenced by its theoretical framework. He contributed ten articles on the rat to the *JCP*. This close connection positioned him in a similar manner to Siegel in the network (Figure 17): close to the rat and the center of the network, and in the least populated side of the cluster. Beyond his influence in the *JCP*, Tryon's work established a new field of behavior genetics – for which he and his student, Jerry Hirsch, provided the name (Hirsch & Tryon, 1956) – that flourished until Tryon took up his other interest, statistics, in the form of cluster analysis.

Structuralist in a Behaviorist Era

The final rat researcher indicated by the network was Paul Thomas Young. Young, completed his undergraduate degree at Occidental College, received his MA at Princeton, and took his doctorate in 1918 at Cornell. Young acknowledged himself as a product of Titchenerian structuralism (Young, 1978); one of the few comparative psychologists who could make that claim. His dissertation (Young, 1918) sought to determine whether or not pleasantness and unpleasantness could be experienced simultaneously. He argued that reports of "mixed feelings," in this instance, were vague, as they blurred the line between the meaning (cognitive) and the

feeling (affective) aspects of experience. He went on to say, "My doctoral thesis established two principles that I believe are valid. First, there is a genuine difference between meaning (cognitive) and feeling (affective). Second, when gut feelings of definite sign (positive or negative), intensity, and duration are experienced they are not felt simultaneously. P and U are incompatible, antagonistic, dynamically opposed" (Young, 1978, p. 42). This line of inquiry defined Young's research for the remainder of his career.

Young's thesis relied on introspective procedures on human subjects. His research was risky, coming at a time when psychology was moving away from the "mentalistic" aura of structuralism and towards the study of objective behaviors. Young acknowledged these competing views of the discipline when describing the controversy between his supervisor, Titchener, and Watson over the definition and methods of psychology: "Watson regarded psychology as an objective biological science. He spurned the introspective method. Titchener regarded psychology as an independent science based on direct observation of human experience" (Young, 1978, p. 42).

Upon receiving his PhD, he moved to the University of Minnesota for a brief stay, before taking a position at the University of Illinois that he held until he retired in 1960. His early research at Illinois was a continuation of his graduate work. He spent his sabbatical in 1926 in Berlin, working with the likes of Köhler, Wertheimer, Lewin, and other Gestalt psychologists, focusing on the physiology of pleasantness and unpleasantness. His work in Berlin led to his change in method and his substitution of the rat for the human. He describes the switch himself in his final journal article:

> I decided to study the likes, dislikes, and preferences of rats for different kinds of food. The subjects were three male rats. The apparatus was an open table covered with

> wrapping paper. The food objects were grains of wheat, oats, corn, and barley purchased on the local market. Several kinds of enclosure were tried for presenting pairs of grains simultaneously.
>
> These early experiments showed that rats are uniform in the preferences they develop. Test foods arrange themselves into hierarchies or transitive series from low to high acceptability. The study demonstrated that preferential behavior can be studied objectively with animals. When I returned to Illinois I continued with a series of studies on food preferences, appetites, and dietary habits (Young, 1978, p. 43).

These experiments were the first of a steady stream that would come out of the Illinois laboratory for the next 42 years. Eleven of those studies were published in the *JCP* by Young using rats. His ensuing experiments on the preferential behavior of rats served to solidify and support his theoretical position that hedonic variables influenced behavior. The most important assertion of Young's theory was that pleasantness and unpleasantness had a neurophysiological basis that could be objectively studied; an assertion that was not accepted until the pioneer study of Olds and Milner (1954) that utilized electrical stimulation on the septal region of the rat's brain.

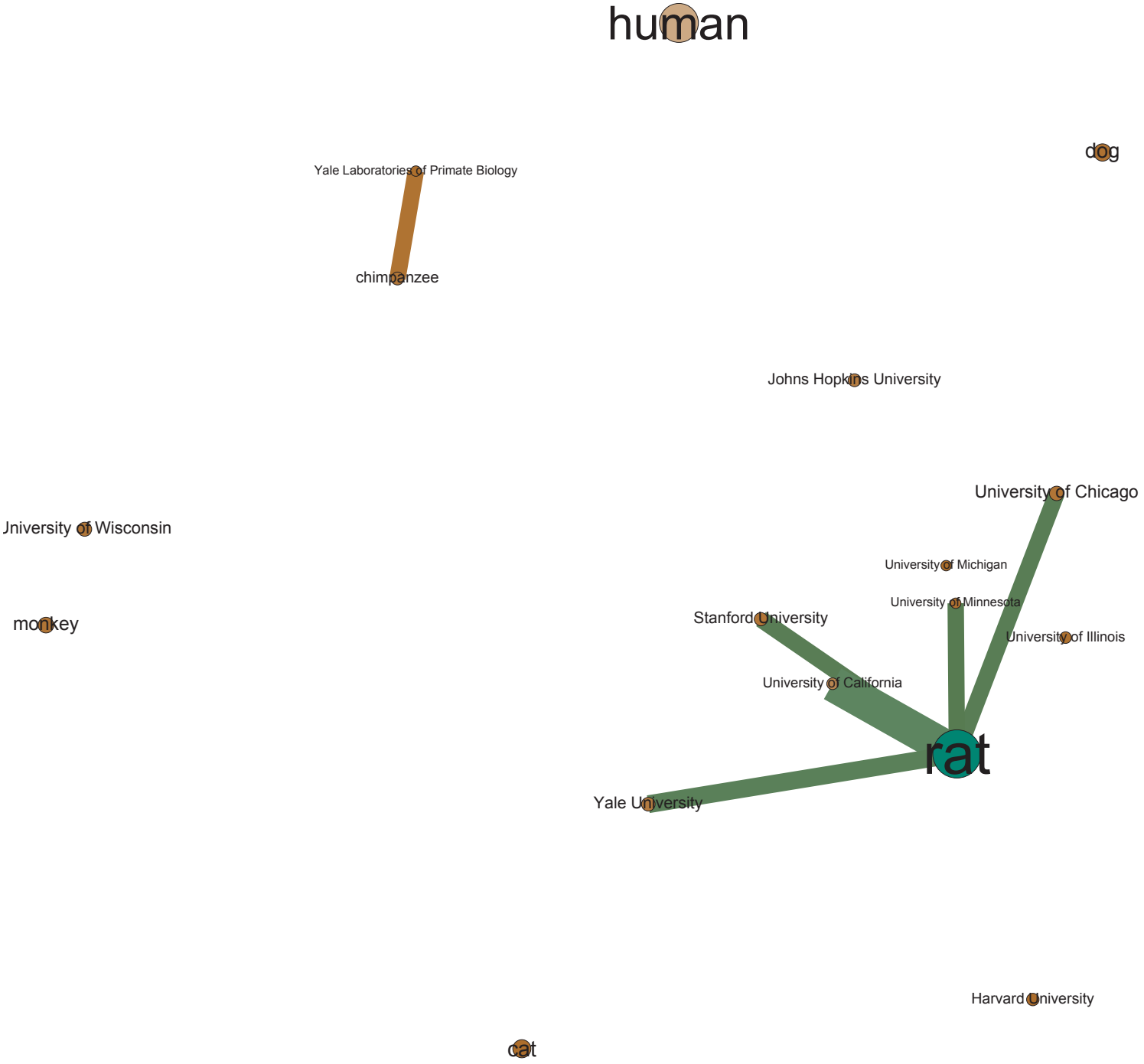
Although Young's research yielded congruent and non-contradictory results with experiments from differing viewpoints (subjective experience, objective animal behavior, neuropsychology), he found himself on the wrong side of the dominant behavioral emphases of the day. Indeed, O'Kelly (1979) argued that Young's association with the "mentalistic aura of dead structuralism" and his promotion of the "hedonic dimension," largely led to the inevitable outcome "that most investigators in this field have neglected, ignored, or denied [his work's] relevancy" (O'Kelly, 1979, 552). This particular case study further emphasized the influence of

neo-behaviorism in the journal. Although Young was prolific in output and was the closest of any author to the rat (see Figure 17), his work never received the attention it deserved because it ran counter to the accepted behavioral theories of the day.

Before concluding this section, some important findings need to be discussed. First, including Maier, Stone, and Beach alongside Muenzinger, Bunch, Siegel, Tryon, and Young, one sees that the plasticity of the rat as a test subject made it a popular model within competing research programs. This variability made it a necessity to "look closer" in order to determine the theoretical frameworks of the authors. Second, the low degrees of the neo-behaviorists represented a generation of psychologists that "inherited" their model organisms from their supervisors (e,g,. Tryon "inherited" the rat once joining Tolman's laboratory). Although only four of the five rat researchers were characterized as behaviorists, these case studies indicated a pattern in which many "dust bowl" researchers conducted their studies under the sponsorship of neo-behaviorism. These findings all support Beach's argument that behaviorism had a strong presence in comparative psychology.

Rat Research Centers

The final part of this chapter sought to determine the most prominent rat research facilities in the *JCP*. To accomplish this, I removed from the network any institution that studied a single organism less than thirty times. The logic behind this decision was that since the rat was such a dominant organism in this network, any institution with thirty or more instances with a single organism would likely be connected to the rat rather than any other species. I argue that if the most prolific rat research centers were behavioristic, then behaviorism was extremely influential in comparative psychology (via the *JCP*). Six institutions were revealed using this method (figure 18): the YLPB, Yale University, the University of Chicago, the University of


human
dog
Yale Laboratories of Primate Biology
chimpanzee
Johns Hopkins University
University of Chicago
Jniversity of Wisconsin
University of Michigan
University of Minnesota
monkey
Stanford University
University of Illinois
University of California
rat
Yale University
Harvard University
cat

California, the University of Minnesota, and Stanford University. Stanford and the YLPB were removed from the analysis due to their appearance in the previous chapters. Of the remaining four institutions, California (sr=74, d=6, wd=90)[33] under Tolman, Yale (sr=37, d=11, wd=66) under Hull, and Minnesota (sr=34, d=3, wd=41) under Heron and Skinner were distinctly behavioristically oriented. Chicago (sr=34, d=13, wd= 51), run by the functionalist, Harvey Carr, was sympathetic to the behavioristic cause, but was less concerned about adhering to the strict framework that had developed.

The University of California[34]

Of the ninety publications that bore the affiliation of the University of California (UC) between the years 1920 and 1950, 74 were directly associated with the rat. The remaining seventeen publications were split amongst five other organisms: chicken, cat, monkey, pigeon, and human. In terms of historical significance, these numbers served as a confirmation of California's prominence within this dominant intellectual trend. Indeed, the Berkeley campus was home to Tolman – who I have established as a prominent neo-behaviorist – and the many of his graduate students, namely, Robert Tryon, Calvin S. Hall, and David Krech, who continued on to their own successful careers in psychology.

The story of the Berkeley was simple: Tolman and his students were, in large part, the reason Berkeley was so highly connected to the rat, and in turn, why it was also the most actively publishing institution. Although Tolman, himself, published only five studies on the rat in which he was first author, he also co-authored one article (as second author) with each of A.E. White (White & Tolman, 1923), F.C. Davis (Davis & Tolman, 1924), C.F. Sams (Sams & Tolman,

[33] (sr=number of studies published on the rat, d=degree, and wd=total weighted degree,)

[34] This included both campuses: the Berkeley campus, and the newly built Los Angeles campus. The fledging status of UCLA meant that only a handful of articles were published in the *JCP* from their department.

1925), and F.M. Geier (Geier & Tolman, 1943). He also served as second author with C.H. Honzik on two occasions (Honzik & Tolman, 1936; 1938). Including these two co-authored publications, Honzik contributed a total of five experimental articles on learning in the rat. Zing-Yang Kuo, perhaps Tolman's most recognized student, conducted eight studies, only one of which was while he was at Berkeley (Kuo, 1922). Tryon's entire academic career was spent at the Berkeley, and thus, all ten of contributions to the *JCP* came under that affiliation. Further than this, Tolman served as supervisor to multiple dissertations in which he received no authorship credit.[35] Predictably, these papers were on maze learning in rats.

Other important figures published for brief spurts of their career under the UC affiliation in this time period. Isadore Krechevsky, later known as David Krech, came to Berkeley in 1932 to study under Tolman, eventually taking his PhD in 1933. Although his work was mostly remembered for contradicting the common held belief that learning was trial and error based and he eventually moved closer to psychobiology, he always considered himself a Tolmanian (Krech, 1974) and he and Tolman remained life-long friends (Ghiselli, Beach, Pickerall & Rosenzweig, 1978). He contributed seven publications on the rat to the *JCP*, however, only one came while at UC; as shown in the previous chapter, his other six experiments were published under the affiliation of the University of Chicago. Calvin S. Hall, who was influenced in his undergrad by Guthrie, and one of the most visible psychologists between the years 1935 and 1975 (Lindzey, 1987),[36] took his PhD at UC under Tolman and Tryon in 1933. Hall published seven rat experiments as first author in the *JCP* in the period between 1920 and 1950. His first two (1934; 1934) came while he was at the UC, the next three were affiliated with Oregon (1936; 1936; 1937), and the final two while chair at Western Reserve (1941; 1942).Finally, Egerton L.

[35] This claim is made off of mentions in footnotes that the author is indebted to Prof. Edward Tolman.
[36] His most well-known research was his later work on psychoanalytic theory and in dreams.

Ballachey, a member of the Tolman laboratory, did what many before him had done: he took his PhD under Tolman, stayed for a brief time at the UC, and then left to explore different research areas. Ballachey had seven publications dedicated to animal learning – an impressive accomplishment for a graduate student, even one in Tolman's prolific laboratory. Only two of Ballachey's papers were published in the *JCP*; both were focused on learning in rats, were affiliated with the UC, and were published in 1934.

Tolman might not have been one of the most active *JCP* contributors, but his influence was felt heavily in the proliferation of his laboratory's research (Krech, Ghiselli & Tuddenham, 1974). In fact, researchers at the UC favored the *JCP* more than any other institution in this era, publishing the most articles in this time frame. Tolman's influence was also seen in the adoption of rat research at Berkeley. With a degree of six, the UC was not a comparative laboratory in the way we have come to define the word: the rat accounted for 82% of the institution's articles. In fact, prior to Tolman's arrival in 1918, the UC, although modest in its output, did not study rats and was highly comparative: from 1913 to 1918, Berkeley researchers published six articles on six different species, none of which were on humans or rats. Tolman researched humans at Harvard, but quickly adopted the rat upon his move to California. And although Tolman's theory was not as wide-spread as Hull's, he ran the most prolific behavioristic department in the *JCP*.

Yale University

Similar to Berkeley, Yale was home to one of the most prominent neo-behaviorists of the era, Clark L. Hull. But unlike Berkeley, Hull was not as influential throughout the entire period the same way Tolman was. Yale, during the 1920s, was under the control of Yerkes, and although Hull took over the Institute of Human Relations (IHR) in 1929, his influence was not fully felt until the 1940s, the peak of his theoretical dominance. Hull, who was at Wisconsin

from 1913 to 1929, conducted most his research while there on hypnosis and aptitude testing. Even though he became interested in behavior in 1930 (Hull, 1952), he did not begin his full-fledged behavior program – which came to be described as "the Yale brand of behavioral theory" (Sears, 1982, p. 1280) – until the mid to late 1930s.

Yale University's degree of 11 indicated that the institution was the second most comparative behind Chicago. However, as was seen in chapter one, the strength of Yale's comparative program came from Yerkes and his Laboratories of Comparative Psychobiology. Moreover, Yerkes, who had studied the rat while at Harvard, was not particularly high on the species, with only two of thirteen articles published on the rat at Yale under his headship. Upon the establishment of the Yale Laboratories of Primate Biology, Yerkes and his comparative program stopped affiliating as "Yale University," eventually leading to Yerkes's desire to sever all ties with the parent institution (Biehn, 2008). Thus began Yale's ascent into, arguably, the premier "hub" of neo-behavioristic research.

The IHR[37] was established nearly the same time as the Orange Park facility and was funded with the same Rockefeller grant. Biehn (2008) argued that both projects were integral to the establishment of the other, perhaps even an "all-or-nothing" situation. Certainly, the promise to the Rockefeller Foundation was that the YLPB and the IHR[38] "were to be cooperative enterprises, in which scientists moved outside of their disciplinary boundaries to address human behavior as a psychological, psychiatric, social and physiological issue" (Biehn, 2008, p. 33). A friend, student, and supporter of Hull's, Kenneth W. Spence, was doing exactly that: he published work on learning in rats under the affiliation of Yale's IHR and work on primates under the affiliation of the YLPB. His research on primates, however, was intended as a response

[37] For the Institute of Human Relations, see Morawski, 1986.

[38] For a more complete description of the founding of both institutions, refer to Biehn, 2008, and Morawski, 1986.

to the implication that reason reigned supreme in apes (Köhler, 1925; Yerkes, 1943) and monkeys (Harlow, 1949). Although these primates clearly displayed reasoning, Spence (1942) a fervent Hull supporter, argued that the chimpanzee's behavior only adhered to the stimulus-response learning processes, and did not display any reason or intellect (Spence, 1942; Rumbaugh, 1997). Of all the researchers at the YLPB conducting similar research, Spence was the only one who denied that the apes displayed reason, sticking with his stimulus-response relationship claim (Rumbaugh, 1997).

In any case, the arrival of Hull and the establishment of the IHR was seen almost immediately within the network: of the 24 articles published in the *JCP* from 1930 to 1939, fifteen were on the rat. In terms of percentages, more than 60% of the total articles dealt with rat behavior, four times the 15% in the previous decade. Although this occurrence might be attributed to Hull's new influence on the direction of the department's research, it was only partly the case. Only seven of the fifteen articles were in some way attributed to Hull or the IHR; three of these articles were published by Hull. The remaining eight were affiliated with the Laboratories of Comparative Psychobiology (LCP), and did not feature any stimulus-response studies. However, the majority of the early IHR affiliated articles came after 1935, indicating the beginning stages of Hull's massive discipline-wide influence. The next decade (1940 to 1950) saw Hull's biggest influence at Yale. This decade consisted of 25 studies with eighteen on the rat (72%). Although there were a few articles that were affiliated with the LCP or just "Yale University," the vast majority of the papers were coming from the IHR. Hull himself only contributed one first authored and one second author article to the *JCP* in this decade. Like Tolman, Hull's influence came in the number of dissertations published on the rat under his purview. Moreover, by the mid 1930s, the main focal points of the IHR and Yale conformed to

Hull's theories and work. And although this only lasted until his death in 1952, his approach was carried on by his student, Spence, who had since moved to the State University of Iowa (Hovland, 1952). Spence himself published six first-author and two second-author articles in the *JCP* with only one of his first-authored articles coming under Iowa's affiliation.

Although sharing similarities to California, Yale fit into its own category. After James Rowland Angell revived the psychology department from the early dark days of the past, the institute early on was beacon for comparative psychobiology before transforming into a "hub" for neo-behaviorism.

University of Minnesota

The early history of the Minnesota psychology department – including Yerkes's tenure as chair and Lashley's years running the comparative laboratory – was discussed in the chapter one. For this chapter, the story at Minnesota begins with the hiring of Yerkes's replacement, Richard M. Elliot. Elliot was chair for thirty-two years, and from the start believed that Minnesota stood the best chance as a behavioristic institution. This resulted in his hiring of the famous behaviorist, B.F. Skinner, in 1936, who was highly productive while at Minnesota (Department History, 2013). The psychology departments at Minnesota and Chicago were interconnected in this period by two threads: first, Lashley had left Minnesota to take a position at Chicago; and second, William Thomas Heron took his PhD from Chicago under the supervision of Carr in 1924. After a brief stay at the University of Kansas,[39] Heron was offered a full professorship at Minnesota, a position he held from 1925 till his death. Although the 1920s was stamped with Lashley's influence, nearly a decade later, Heron had taken control of the department, churning out new doctorates(?) and decidedly changing rat research at Minnesota from Lashley's

[39] Heron graduated with an MA at Kansas working under the known comparative psychologist Walter S. Hunter. He returned in 1924 to take up a position as assistant professor, presumably working once again with Hunter.

psychobiological perspective to a more traditional "behavioristic" approach. Interestingly enough, Heron had been at Minnesota for a decade before he began publishing in the *JCP* in 1935, which closely coincided with the arrival of B.F. Skinner in 1936.

Minnesota's degree of three made it the least comparative department of the four. Only the rat, human, and chicken were being studied in the 31 year period with the rat accounting for 83% of the publications. This new wave of rat research coming out of the Minnesota laboratory closely resembled the prevailing behavioristic approaches of the day. After the last of Lashley's students had cleared through (some being recruited by John E. Anderson[40]), a changing of the guard began. With Heron and Skinner working together at Minnesota, Elliot's vision of a behavioristic department was complete. As was mentioned earlier, Heron was at Minnesota a decade before he first published in the *JCP*. This was no doubt due to his early career preference of using more general journals as an outlet to disseminate his work (*Psychology Bulletin,* and *Pedagogical Seminary*). Also, many of his early publications were descriptions of various apparatus, and thus, did not qualify for this analysis. Nonetheless, his first students started publishing their dissertations in 1933 (E.A. Rundquist, 1933). Heron's (d=1, wd=6) first experimental publication in the *JCP* came in 1935, and he continued to publish throughout the

[40]Anderson's arrival at Minnesota in 1926 coincided with two things, the founding of the Institute for Child Development in 1925, and the appointment of Florence Laura Goodenough as assistant professor at Minnesota, in 1925. Both child development psychologists were most likely drawn to Minnesota based on the newly formed Child Institute. Indeed, Anderson was called to Minnesota to organize a research program in child development (Templin, 1968). Along with the presence of Goodenough, the Institute had a fairly distinct female connection: Anderson graduated with a BA at the University of Wyoming in 1914, and there collaborated with the Wyoming female psychologist, June Downey, to publish a paper in 1915. He finished his education at Harvard, where he took his MA in 1915, and his PhD in 1917, both under Münsterberg. After serving in the war for two years, he was appointed instructor at Yale in 1919, teaching until his move to Minnesota in 1926 (Templin, 1968). The three female students, Shirley, Hubbard, and McCarthy, who were previously studying learning in rats with Lashley, all changed their research interests towards child development after Lashley left. Anderson (1932) only published once in the *JCP*, an article on the effects of nutritive conditions on learning in the rat. Perhaps he believed the *JCP* was more of an appropriate outlet to publish his rat work, as he preferred other periodicals for his and his students' work on child development (namely the *Psychological Bulletin*, and *Child Development*). Indeed, Goodenough, Anderson, and their students stopped publishing in the *JCP* after 1932. Nonetheless, his influence was strong enough to recruit Lashley's former students over to child development research.

period of 1935 to 1950 for a total of six first-author and two second-author publications. Although Skinner did not publish experimental research in the *JCP* during this period, his arrival in 1936 sparked an active collaboration between Heron and Skinner up until 1940, with the majority of their co-authored articles published in *The Psychological Record*.

As a product of Harvey Carr, who ran one of the most productive laboratories at Chicago, it is unsurprising that Heron was producing a new graduate yearly from 1937 to 1942. Similarly to Tolman's and Hull's (and later, Carr's) department, Heron's department was characterized by many single ("one and done") dissertation publications by different researchers on the rat's learning behavior. In this period, Heron's most prolific student was Kenneth MacCorquodale (d=1, wd=3), who published three first-author publications and two as second-author. All of MacCorquodale's articles were on the rat, and all were distinctly pro-Hullian (MacCorquodale & Meehl, 1948; Meehl & MacCorquodale, 1949), insofar as they attacked Maier's research on "reasoning," and Tolman's theories on (no) reinforcement and latent learning. Reinforcement, of course, was a main tenet of Hull's and Skinner's theoretical frameworks. The work by MacCorquodale and Meehl that attempted to disprove other theories while supporting their own characterized the direction research took at Minnesota from 1935 on.

The story of the Minnesota psychology department is an appropriate case study on the impact of behaviorism on comparative psychology. The department had distinct comparative roots; the department's first chair was the animal psychobiologist Robert Yerkes, and his first hire was the comparative psychobiologist Karl Lashley.[41] Moreover, Heron was a student of the functionalist and comparative psychologist, Harvey Carr. Yet, the new chair, Elliot, understood the situation at Minnesota better than Yerkes ever did. He realized that to remain relevant in the current literature of the day, they could ill afford a purely comparative (and perhaps outdated)

[41] Throughout his career, Lashley conducted studies on at least five different species.

department and needed to find their place within the behavioristic framework. The 1920s was a time when Watsonsonian behaviorism was abandoned and other forms of "radical" behaviorism were being exposed. Indeed, Lashley conducted some of the experiments that destroyed these stimulus-response theories. This bred a time ripe with competing theories on the "lawfulness" of behavior, and no single theory was the "standard."[42] With the rise of Tolman, Guthrie, Hull, and other theory builders, behavioristic theory was beginning to reunify. Elliot knew that psychological theory was in a transition phase, so he, and the department under his purview, embraced this change by bringing over Skinner from Harvard, in 1936, to join the incumbent, Heron. This move indicated Elliot's success in turning Minnesota from a shrinking comparative department, to a growing behavioristic one run by Heron and Skinner.

University of Chicago

The second chapter established Lashley's lasting influence on the research coming out of the Chicago department. Furthermore, Chicago's appearance as one of the most prolific rat research centers also owed much to Lashley's tenure there from 1929 to 1935. However, Chicago's reputation as one of the most productive and comparative departments in this era was due mostly to functionalist and comparative psychologist Harvey A. Carr, who took control of the department in 1922 and remained in charge until 1938. Like Lashley, Carr shared Watson as a mentor, albeit it at Chicago rather than Johns Hopkins. Also like Lashley, Carr had an affinity to the rat as a research subject.

Prior to coming to Chicago, Carr received his MA from the University of Colorado under the purview of Arthur Allin, in 1902. It was Allin who introduced Carr to psychology. Often

[42] Many considered Hull's theory as the "standard," but this did not take hold until the 1940s. (Benjamin, 2007).

considered the last functionalist, Carr[43] took his PhD under the likes of known functionalists[44] John Dewey and James Rowland Angell, as well as John Watson. It seemed, however, that Carr was most heavily influenced by Watson. Indeed, in 1908, when Watson moved to Johns Hopkins, he handed the reins of the animal psychology program he developed during his time at Chicago over to Carr (Pillsbury, 1955). After taking control of Watson's laboratory, the newly graduated Carr set out to work on the white rat. His main area of research focused on the different senses the white rat relied upon when navigating a maze.[45] In his APA presidential address in 1927 entitled "Interpretation of the Animal Mind," he asserted his stance within the discipline: "I am somewhat of a behaviorist in the field of animal psychology, although I do not class myself as such so far as human psychology is concerned" (Carr, 1927, p. 104). This

[43] Harvey Carr joined other members, Dewey, Angell, and George Herbert Mead, to make up the Chicago school of functionalism. Another group of functionalists were headed by James McKeen Cattell at Columbia, and also included E.L. Thorndike, and Robert S. Woodworth (Green, 2009, Whitely, 1976).

[44] Near the close of the nineteenth century, the reaction time tests that Wundt relied on were beginning to fall apart, threatening to undermine his entire physiological psychology program. Wundt predicated his theory on their being a single "normal" mind, and his inconsistent reaction time data indicated individual differences that, of course, disproved Leipzig's position (Knight, 1994). Wundt's assistant, Ludwig Lange (1886) restored order momentarily, until James McKeen Cattell, Wundt's American assistant revealed that some individuals have a natural propensity to respond quicker. A loyal proponent of Wundt, Edward B. Titchener, defended Lange's work (which refuted the work of Cattell) on the bases that certain individuals were not experienced enough at the task, in which case they lacked the "mental disposition" to respond correctly. This led to a verbal debate between Titchener and James Mark Baldwin, who in his own studies, found none of the consistency Titchener had claimed (Green, 2009).

At the same time, James Rowland Angell and Addison W. Moore, at the University of Chicago, conducted the most wide-ranging set of reaction time tests yet, and argued for a "dynamo-genetic" theoretical framework. They purported that a response to a stimulus was not merely a reflexive act explained through physiology, but included the attention the observer placed on the sensory form elicited by the stimulus. For them, the time it took for an individual to respond was directly related to how fast an observer shifted their attention from one sensory form to another. A common example is the difference in time it takes one to shift their attention from the ear to the hand when responding to an auditory stimuli; the ear, of course, being the more habitual relationship (Angell & Moore, 1896, p. 252; Green, 2009). Not long after, Chicago professor John Dewey, Angell and Moore's mentor, published functionalism's founding document, "The Reflex Arc Concept in Psychology" (Dewey, 1896), that outlined the theoretical foundation developed by his students.

The story of Watson's tenure at Chicago is best described by Kerry Buckley (1989). A student of Angell's, Watson took his PhD by the young age of 25, in 1903. Learning under Dewey and Angell, the new doctorate Watson turned down many offers and took an instructorship position at Chicago where he continued to work with the latter (Benjamin, 2007). Functionalism, with its concern on learning as an adaptive means to a changing environment provided Watson a unique position to study with animal subjects.[44] Animals created the ideal subjects since they could be bred and allowed them to be in "controlled" conditions. They also turned out to be great learners (Benjamin, 2007). Watson established an animal psychology program at Chicago and this animal research came to dominant functionalism, behaviorism, and comparative psychology.

[45] Carr used a maze he devised himself that was widely used (Pillsbury, 1955).

position was revealed in Carr's discussion of consciousness in animals. He believed consciousness in animals can only be decided if animals and men are shown to respond similarly, a claim he was uncomfortable making. In fact, Carr expressed caution in the anthropomorphizing of animals in many of his papers; he acknowledged this hostility to anthropomorphic interpretation as a precipitate of the intellectual environment he was reared and nurtured in at Chicago (Carr, 1927). Even though he personally approved of behaviorism, he believed experimental evidence could be attributed only to the animal that was under study, and could not be a surrogate to the understanding of the human mind. With Carr's doctrine intact, and his emphasis on animal research, the Chicago laboratory rarely researched human subjects. Indeed, Chicago published only four articles on the human in the period of 1920 to 1950, which was considerably less than the 34 on the rat.

Carr's career research centered around comparative psychology, space perception, and learning. As such, these three topics formed the basis of the experimental work stemming from the Chicago psychology department, with a distinct focus on non-human organisms. From the years between 1920 and 1950, Carr (d=1, wd=1) published only one article as single author (1926), and co-authored one with Siegfried Maurer (1935) in the *JCP*. In this scenario, the network was only able to document Carr's contribution to the journal, and thus, the visualization was unable to portray his individual influence in comparative psychology. This is unsurprising considering Carr's importance in the discipline has mostly been linked to the influence he had upon his students while at Chicago, and later when they were directing their own laboratories. Indeed, he was an institutional or "organizational" man rather than a laboratory scientist (Pillsbury, 1955). Of the 131 degrees that were awarded while the department was under the purview of Carr, the majority benefitted in some capacity from his advisorship (Koch, 1955).

Moreover, he was directly responsible for 53 doctorate degrees; 18 in animal work, 29 in learning, and 6 in space perception (Pillsbury, 1955).

Similar to the Berkeley, Yale, and Minnesota departments, Chicago's contribution to the *JCP* was mostly through many single dissertation publications by different researchers. Only two researchers, Loh Seng Tsai and Siegfried Maurer, published more than one article in this time period under the affiliation of Chicago (with the exception of Lashley), and both were in some way tied to Carr. Tsai (d=1, wd=2) took his PhD at Chicago in 1928, and joined the faculty ranks in 1929 (Lindley, 1993). He published two articles on the rat in the *JCP* as a single author (1930; 1931), and one as a co-author with Siegfried Maurer. Maurer (d=1, wd=6) was a graduate of Rush Medical School, and a physician that practiced in Chicago from 1924 to 1952. He published six articles on the rat, but was especially prolific in the year 1935 when he published five articles as single author, including one in which Carr was co-author. Both Tsai and Maurer's research focused on maze learning in rats.[46]

Under the purview of the functionalist and comparative psychologist Carr, rat research at Chicago never strictly adhered to the stimulus-response framework that classified many psychology laboratories at the time. However, it appeared that as a functionalist, which has been closely linked to behaviorism (Green, 2009), Carr and his department were sympathetic to behavioristic theory. Interestingly, after Carr's retirement in 1938, only two articles were published on the rat at Chicago during the period of 1939 to 1950, and both of these articles were published in 1939 by K.W. Bash.[47] Apparently, Carr's preference for the rat was the driving force for the rat research generated at Chicago, or at least the rat research published in the *JCP*.

[46] Both researchers began publishing in the *JCP* by 1930, one year after Lashley had accepted a professorship to Chicago.

[47] Both of these articles were received for publication in November of 1938, providing credence to the claim that Carr was the driving force behind rat research at Chicago (Bash, 1939; Bash, 1939).

However, unlike Yale which was comparative prior to becoming a rat research center, Chicago, under the self-proclaimed comparative psychologist Carr (Pillsbury, 1955), had remained comparative throughout. In fact, with a degree of 13, Chicago studied the most diverse array of subject species of any institution publishing in the *JCP* from 1920 to 1950. Among these thirteen species were the dog, cat, chicken, mice, salamander, human, frog, and monkey.

The histories of all four of these "hubs" for rat research pointed to three things: first, the proliferation of the institution's research was attributed to the influence of one or two prominent laboratory "heads"; second, the majority of the rat research came in the form of single dissertation publications, and thus, resembled a "factory-like" approach to churning out new doctorates; and third, a low degree indicated that the institution was more likely to have adopted a behavioristic approach. Yale under the guidance of Hull,[48] California under Tolman, and Minnesota under Heron and Skinner all had low degrees and had fully invested in the rat as a subject to study behavior via learning (or performance in Tolman's case). Although not opposed to behavioristic theory, Chicago, with a degree of thirteen, was the only one of the four institutions not behavioristically oriented, and thus, illustrated that the least comparative departments were most likely to have been behavioristic institutions.

Taken collectively, this chapter has provided credence to Beach's claim that the influence of behaviorism in the *JCP* was strong and pervasive, and went far beyond the individual contributions of the neo-behavioristic giants. The fact that most of the rat research in the *JCP* was emanating out of behavioristic institutions further emphasized the earlier argument that the rise of the rat in the 1930s was an artifact of neo-behaviorism and the theories of the neo-behavioristic giants, Tolman, Hull, and Skinner. Moreover, of the five authors depicted by the network to be most prolifically studying the rat (Muenzinger, Bunch, Siegel, Tryon, Young),

[48] All of Yale's comparative work came under Yerkes.

only Young was characterized as something other than a neo-behaviorist. Muenzinger, Bunch, and Siegel, and to a lesser extent, Tryon, were all considered “dust bowl empiricists” and these individuals were only a few examples of the broader acceptance of behavioristic theories by experimental psychologists. Although Beach’s claim that behaviorism had essentially replaced comparative psychology was misinformed, he was correct in drawing attention to its impact on comparative psychology.

Conclusion

There has been no consensus by historians on the successes and failures of comparative psychology, or even if there was a fully active comparative discipline after the 1920s. One common thread amongst all histories on comparative psychology was the widespread adoption of the Norway (white) rat, and the maze learning studies they featured in. However, Donald Dewsbury (1984) has shown in his book, *Comparative Psychology in the Twentieth Century*, that comparative psychology cannot be reduced to only the organisms studied, but its history needed to include the full breadth of the discipline. Achieving this breadth, Dewsbury contended, could not be done via the frequency counts of organisms studied, employed first by Theodore Christian Schneirla, and expanded upon by Frank Ambrose Beach and many other historians. Furthermore, while Dewsbury acknowledged that rat research was predominant in comparative psychology, he argued that focusing on this aspect alone masked the importance of individual comparative research papers (on both rats and non-rats) and textbooks contributed to the discipline. Moreover, Dewsbury argued that although the ranks of comparative psychologists have never been many, the few were prominent and highly influential within the broader discipline of psychology. It was these few, multi-generational prominent psychologists that Dewsbury believed established and continued the "rich history of a broad-based, evolution-oriented science of behavior" (Dewsbury, 1984, p. 2). He chronicled the history of comparative psychology in decades, claiming that the 1910s and 1920s were a time of development, transition, and maintenance in comparative psychology, accomplished from the research of a few "first generation" comparative psychologists. Dewsbury then argued that the 1930s saw comparative psychology prosper (a claim that was not supported by many historians), mostly due to the

arrival of the "new generation" of comparative psychologists. He further purported that this new generation helped sustain the vigor developed in the 1930s into the 1940s.

Beach understood this rat phenomenon less optimistically than Dewsbury, seeing it as effectively replacing comparative psychology with behaviorism. Indeed, aside from the pigeon, which was popularized by Skinner and his "radical" behaviorism in the 1960s, the rat has historically been recognized as the model organism in behaviorism. Moreover, the rat's rise to prominence as a subject species began in the 1920s but reached its peak use in the 1930s, coinciding with the establishment of numerous competing behavioristic theoretical frameworks. Thus, it was a short leap for Beach to connect the mounting rat studies in comparative psychology to the advent of behaviorism. However, as was discovered in this thesis, the rat was not an exclusively behavioristic organism, but the versatility of the rodent made it a staple in developmental, neurological, and physiological studies as well. Additionally, the animal was used equally in the research programs of behaviorists and non-behaviorists (what I have termed as psychobiologist). Beach was not unaware of the diverse use of the rat, and did not claim that all rat research was behavioristically oriented. To guard against this generalizing statement, he attempted to decipher the different topics of research and found that the rat was not only used in staggering numbers, but learning studies reigned supreme in comparative psychology, and these learning studies typically featured rats working through variously shaped mazes. This was the bases in which Beach claimed that behaviorism had essentially replaced comparative psychology.

There were obvious methodological differences in Beach's and Dewsbury's studies, and this led to the use of different data. Beach employed a frequency analysis of the organisms used in the *Journal of Comparative Psychology*, and coded seven distinct categories of research.

Beach's data included only odd numbered volumes of the *JCP* from 1911 to 1950, predicating his entire argument on this subset. Dewsbury employed a fuller history of comparative psychology, including in his data not just publications in the *JCP*, but also the many influential textbooks that surfaced, the articles in other, more general psychology periodicals, conferences and other important events, as well as prominent institutions, individuals, organisms, and groups/councils. It was clear upon investigating the literature that the claims made by each author were legitimate, and it was the reconciling of the simultaneous existence of both claims that animated this study.

In my analysis, I attempted to bridge the "data" gap between these two important critiques on comparative psychology in order to view each claim via the same lens. Although I similarly only used data from the *JCP*, I employed a fuller analysis of the journal, including all of the metadata from 1911 to 1950. I also expanded the data to include not just the model organisms and the studies they featured in, but also prominent researchers and institutions. The inclusion of the most prominent researchers and institutions allowed for the extrapolation of important historical information not available from the metadata. This approach allowed me to repair methodological shortcomings in both studies. Whereas Beach only used part of the data, and focused his entire study on numbers and proportions alone, I used all of the data in the same time period whilst simultaneously employing traditional historical methods. Dewsbury, although expansive and thorough in the breadth of his data, focused the majority of his book on the most important, cause for celebration, moments in the discipline's history. This approach failed to capture the broader disciplinary trends that were present when viewing frequency counts and proportions. Thus, I employed a network analysis – a common tool in digital history – for this study in an attempt to visualize this data in a way that included both the frequency counts and the

historical narrative of comparative psychology. This allowed me to conceptualize the broader organism trends and discuss the specific historical actors responsible for maintaining comparative psychology. I organized the data into two different types of networks: one depicting the relationship between organism and scientist; and the other depicting the relationship between the organism and the institution conducting the research.

It is ironic that the most advantageous feature of this study (including all of the data in a particular time period) was perhaps the most limiting. The sheer number of data points made not only the networks very large and dense, but the spreadsheet incomprehensible without the "find" function. With such dense networks, there is no shortage of storylines to follow, and one could find themselves writing a superficial overview of the discipline. To refrain from falling into such traps, I utilized the network visualization in conjunction with an extensive literature review. The information gathered from the network could be broken into two components: measures of degree centrality, and the structure of the network. Degree centrality is a quantitative measure that is used at the individual node level that consists of degree and weighted degree. Degree measures the number of distinct connections a node had in the network, and indicated how many organisms a particular scientist or institution studied, and conversely, how many scientists and institutions studied a particular organism. Weighted degree is the total number of connections a node had, and designated the most active scientists and institutions, or the most actively studied organisms. The structure of the network was analyzed as the total shape of the overall network, which was useful in determining different communities of scientists/institutions and organisms, and by the individual positioning of nodes within the network. Only the position of nodes that represented scientists were analyzed in this thesis and their individual positioning was highly dependent on their measures of degree centrality. Highly comparative scientists (high degree)

were most likely to be surrounded within the network, as these individuals connected multiple organism communities. Actively publishing individuals (high weighted degree) were likely to have been represented by bigger nodes and most closely positioned to their model organism.

Furthermore, I utilized the network visualizations in two different ways in my study, as a guiding tool and as a way to reify existing historical facts. This is an important distinction which speaks to the versatility of the technique. To unpack this more, I used the network to determine which scientists and which institutions were most closely connected to the rat, and thus, used the network to "guide" me to the correct answer. I also used the network to confirm established historical events, which took the form of tracking certain scientists (Yerkes, Lashley, Stone, etc.) and the different institutions they were affiliated with. In this case, the network did not provide original information, but served as a visual confirmation of the pre-established historical facts. These are just two of the many ways networks can be interpreted.

To supplement these quantitative and structural measures innate to network analysis, I employed knowledge gained from other historical accounts on comparative psychology. Since there was such a rich historiography on behaviorism, it was not difficult to code certain "check points" of the movement – the rat and behaviorist giants like Tolman, Hull, and Skinner – that would help guide which parts of the network to focus on and how best to assess Beach's claim. It was trickier in determining which parts of the network to focus on to assess Dewsbury's claim, since there been little consensus on the term "comparative psychologist" or even "psychologist," for that matter. Thus, I used the term "psychobiology," as defined by Robert Yerkes and Knight Dunlap, the two founders of the journal under consideration, to represent the opposite of a behaviorist, or a non-behaviorist. I also included the term "comparative" to the tag of psychobiologist to imply an individual's documented importance in the comparative discipline.

The "check points" that designated a "comparative psychobiologist" was the inclusion of some aspect of biological importance in the study of behavior, as well as a non-adherence to a behaviorist approach. As was found in the thesis, a comparative approach (high degree) was not always the best indicator (e.g., Maier and Beach).

Combining the quantitative measures of the network with established historical precedent led me to the finding that both Beach's and Dewsbury's claims on the status were centrally correct, but both slightly flawed in their belief that the two claims were mutually exclusive. For Beach's claim to have been fully correct, comparative psychology would indeed have disappeared and been completely replaced by behaviorism (rat learning). This, however, was not the case as a handful of prominent comparative psychobiologists maintained a tradition of comparative research throughout the time period. And however much this reifies Dewsbury's claim, his history fails to fully examine the large scale behavioristic trends that seriously impacted the comparative discipline. Thus, although Beach was extreme in his pronouncement on the status of comparative psychology, he was signaling attention to what was indeed a growing phenomenon within the discipline. Interestingly, lost in these competing arguments were the similarities between the two groups of scientists: both studied the rat; both were committed to separating themselves from the mentalistic aura of psychology's past; and both focused on objective laboratory experiments measuring overt behaviors (although psychobiologists did conduct field studies).

Of course, too many data points was not the only limitation in this study. One other one was that the nodes represented three different actors (organism, scientist, institution), making it illogical to compare the degree centrality of dissimilar nodes. For instance, one cannot compare the degree of Harvard University (no matter how high it was) with the degree of the rat because

their nodes are representative of different objects. This is an issue because the network views these nodes as similar, even though they are not. Dealing with this limitation was fairly straightforward; one just has to avoid drawing conclusions about dissimilar nodes. However, the benefits of using a network analysis for a study like this far outweighs the potential issues. Why the network was tailored to the research in this thesis was because it allowed an opportunity to bridge the two claims by showing a more complex and complicated picture than frequency counts alone, while still keeping the important functions frequency counts serve. Additionally, with the inclusion of extra data points (authors, and institutions), and the particular connections they made, the networks provided a visual snapshot of the breadth and complexity that Dewsbury argued was the nuance necessary to properly assess the history of comparative psychology.

Another determination that needs to be addressed is whether or not tracking multispecies networks of researchers and their chosen experimental populations was a reliable technique for identifying 1) their theoretical orientations; 2) their institutional affiliations; and 3) some other group or communal identity. Using the *JCP* as an example, it appeared that the answers to these questions were varied. I was unable to unequivocally identify an individual's theoretical orientation using the network because the rat was such a versatile organism that both behaviorists and psychobiologists studied the animal. However, the organisms a particular scientist studied did provide some predictive value in determining a theoretical orientation. For example, if an individual was studying multiple organisms, it would be safe to predict that that scientist, comparative in their research approach, was most likely to have been a psychobiologist over a behaviorist. Even with this predictive value, one still needed to "double check" a scientist's theoretical disposition to be certain.

Since the network was meant to portray the dyadic relationships between scientists and organisms and institutions and organisms, this was not the best technique for identifying an individual's institutional affiliation. The best way to determine this would still be mining it directly from the published article. Again, however, there was some predictive value in connecting the scientist's organism of choice with the institution's organism of choice. For example, as was exemplified in the thesis, the Yale Laboratories of Primate Biology, the University of Wisconsin, and Indiana University were the foremost institutions researching the chimpanzee, the rhesus monkey, and the dog, respectively. Thus, if a scientist had a particularly close connection to one of these organisms, it would be safe to predict their institutional affiliation. Similar to an individual's theoretical orientation, the only way to confirm a scientist's institutional affiliation was through consulting external resources (i.e., the article themselves).

The network was a reliable technique in determining an individual's identity in a group or community. In fact, I would argue that one of the greatest advantages of the network is how it organizes individuals into communities based on their preferred model organisms. Thus, all individuals studying the rat were positioned close to the rat node in the network, and effectively became its own cluster or community. This was the same for other organisms as well; all monkey researchers were positioned close to the monkey, forming its own cluster. Of course, the size of a particular cluster or community depended upon the number of scientists connected to that organism. Understanding that the network organized the data in such a fashion, one is able to make an unequivocal claim about a particular researcher's communal affiliation simply by looking at their position within the network. However, this "communal" affiliation did pose some problems when two individuals with contrasting theoretical orientations were organized in the same community. To illustrate this point, Frank Beach's close connection to the rat meant

that he was integral to the overall rat community. If we take this close affiliation with the rat community at face value, we run the risk of misattributing Beach as a neo-behaviorist, when he was, in fact, a psychobiologist. This tells us two things. The first and most obvious is that there are some innate flaws with the technique insofar as its communal affiliation is dependent on species studied alone (this could also be an artifact of my coding decisions). The second, and more telling, is that these two groups (psychobiologists and neo-behaviorists) were not as different as once presumed. Both groups conducted behavioral studies on the same organism(s) (mostly the rat), both could be considered experimental psychologists, and finally, both published their research in the *JCP*.

With the findings in this thesis spelled out above, one could extrapolate that the simultaneous existence of competing behavioral research programs created an ambiguity within the comparative discipline, and was perhaps the reason why there is no consensus on the discipline's history and how the term "comparative psychologist" should be used. However, the importance of this thesis went beyond the historical finding that the two arguments were not mutually exclusive and its significance in reassessing the status of comparative psychology. This thesis attempted to employ a novel approach to the assessment and writing of history, and thus, had important methodological implications. I have shown that the networks provided a platform to not only view and incorporate all the data into an analysis, but it allowed the historian to meld quantitative measures with the actual historical facts or, in the case of Beach and Dewsbury, the discrete frequencies with the complex back-story. Moreover, network analysis was a reliable technique in determining particular group or community affiliations. And although there are many shortcomings innate in these techniques, they are easily managed if the network is used as a supplemental tool, and not as an end in and of itself.

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Appendix A (Tables)

Table 1 – Organisms (1911-1950)

Degree	Weighted Degree
Rat (348)	Rat (747)
Human (127)	Human (185)
Monkey (39)	Monkey (61)
Cat (36)	Dog (55)
Dog (35)	Chimpanzee (54)
Chimpanzee (27)	Cat (42)
Chicken (26)	Chicken (34)
Guinea Pig (17)	Pigeon (27)
Pigeon (16)	Guinea Pig (22)
Mice (12)	Ant (18)
Rabbit (11)	Mice (15)
Bird (11)	Bird (15)
Frog (8)	Wasp (13)
Wasp (8)	Rabbit (11)
Ant (8)	Frog (8)

Table 2 – Scientists (1911-1950)

Degree	Weighted Degree
R.M. Yerkes (8)	N.R.F. Maier (25)
H.F. Harlow (7)	C.P. Stone (21)
V.E. Shelford (7)	M.E. Bunch (16)
A.H. Maslow (7)	H.F. Harlow (15)
C.P. Stone (6)	H.M. Johnson (15)
S.B. Vincent (6)	K.S. Lashley (14)
S.J. Holmes (6)	W.S. Hunter (13)
W.N. Kellogg (6)	A.H. Maslow (12)
F.T. Perkins (5)	K.F. Muenzinger (12)
K.S. Lashley (5)	R.M. Yerkes (12)

H. Frings (4)	P.S. Siegel (12)
G.V. Hamilton (5)	F.A. Beach (11)
W.F. Grether (5)	K. Dunlap (11)
A.S. Pearse (4)	P.T. Young (11)
K. Dunlap (4)	W.N. Kellogg (11)

Table 3 – Organisms (1911-1930)

Degree	**Weighted Degree**
Human (71)	Rat (119)
Rat (70)	Human (100)
Cat (14)	Monkey (17)
Chicken (13)	Chicken (16)
Dog (11)	Dog (15)
Monkey (11)	Cat (14)
Guinea Pig (7)	Guinea Pig (9)
Rabbit (7)	Bird (9)
Bird (6)	Wasp (7)
Fly (5)	Rabbit (7)
Wasp (5)	Fly (5)
Beetle (5)	Ant (5)
Frog (4)	Dove (5)
Paramecium (4)	Beetle (5)
Fish (4)	Frog (4)

Table 4 – Organisms (1931-1950)

Degree	**Weighted Degree**
Rat (293)	Rat (628)
Human (61)	Human (85)
Monkey (30)	Chimpanzee (50)
Chimpanzee (25)	Monkey (45)
Dog (24)	Dog (40)
Cat (24)	Cat (28)
Chicken (14)	Pigeon (24)
Pigeon (14)	Chicken (18)
Guinea Pig (10)	Guinea Pig (13)
Mice (10)	Ant (13)
Bird (5)	Mice (12)
Pig (4)	Wasp (6)

Rabbit (4)	Bird (6)
Wasp (4)	Pig (5)
Turtle (4)	Horse (5)

Table 5 – Institutions (1911-1950)

Degree	**Weighted Degree**
Harvard (22)	California (96)
Chicago (22)	Hopkins (95)
Wisconsin (21)	Chicago (72)
Johns Hopkins (15)	Yale (70)
California (12)	Wisconsin (63)
Yale (12)	Harvard (55)