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Original Paper

The Constraining Influence of the Revolutionary on the Growth of the Field

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Abstract

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This article draws attention to a pattern of development within science and other intellectual research communities that has received virtually no mention **AQ1** **AQ2** . We propose that subsequent dominance of a research community by a figure responsible for significant innovation often delays progress in the field **AQ3** **AQ4** . During the period in which the revolutionary continues to influence research in a community, far too frequently the effect is to freeze progress within the limited directions which the revolutionary sanctions **AQ5** **AQ6** .

Keywords

Revolutionary
Scientific revolutions
Growth of science
Constraining influences
Darwin
Freud
Chomsky
Fischer

SECTION 1

1. Introduction

This article draws attention to a pattern of development within science and other intellectual research communities that has received virtually no mention. We propose that subsequent dominance of a research community by a figure responsible for significant innovation often delays progress in the field. During the period in which the revolutionary continues to influence research in a community, too frequently the effect is to freeze what Kuhn called normal science ‘puzzle solving’ into the limited directions which the revolutionary sanctions. This constrains theorizing to speculation of a type which the revolutionary advocates, and prevents attempts at reconceptualization almost entirely. Having made one revolutionary advance, the dominant figure effectively prevents subsequent progress until he or she either retires from the field or dies. “Science advances one funeral at a time” (Planck, 1949), After his or her initial contribution, the continued presence of an innovative or revolutionary figure frequently stifles progress rather than aids it. We document four cases of this phenomenon as of signal consequence in the history of science and philosophy. We stress at the outset that this is an overview, designed to press the plausibility of our thesis rather than examine it in complete and necessary historical detail. We proceed in section one with a more thorough explication of the problem. Section two examines four examples of just how these revolutionary thinkers often constrains development and growth. In section three we broaden our argument by demonstrating the breadth and general scope of the problem by sketching the import of this phenomenon at the intersection of sociology, philosophy, and psychology of science in light of prominent theories of scientific methodology, and the insufficiency of presumptive methodologies to explain such episodes **AQ7** .

SECTION 1

2. The Problem: Neglected Factors in Revolutionary Episodes

The factors involved in the creation of a revolutionary in a research community can be divided into (at least) three rough categories: psychological (involving primarily the cognitive structuring of the ‘great man’), sociological (involving primarily the group dynamics of the research community), and rhetorical (involving the argumentative interaction of the individual and the research community). In all categories there are factors which lead to the continued presence of an innovative figure constraining subsequent development in the field **AQ8** .

The psychological factors involved in successfully arriving at and then forcing upon a research community, ideas which run counter to entrenched traditions paint a picture of the revolutionary that is increasingly commonplace in historical analysis. Such thinkers must be able to work in relative or complete isolation, often with little real contact with or encouragement from more than a handful of others. One must have a fundamental insight, nurture it into shape as a viable scientific idea, then have the courage to present it before an indifferent or even hostile audience, as well as the tenacity and aggressiveness (to say nothing of cunning) to persist until the view wins adherence. There is a good blend of Winston Churchill and bulldog in the successful revolutionary. He or she must have *and maintain* the courage of their convictions in the face of constant, often all but overwhelming opposition. One must discount or ignore the ideas of one’s opponents, and believe in the greater survival value (indeed, the complete truth) of one’s own. To be revolutionary one must, in short, develop a novel and informative conception which runs contrary to the grain of one’s research fellows; to be successful one must persevere to see that conception triumph over its opposition. Being successful rewards the revolutionary for ignoring (often quite cogent and compelling) counterarguments proposed by rivals. Thus having seen his or her views survive in the process of becoming the new orthodoxy, those factors which initially aided the revolutionary in

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establishing his or her case will be brought to bear against all subsequent dissent, especially potentially revolutionary positions put forth by rivals in the community against the hard won new orthodoxy. Having been rewarded in the past for discounting others' viewpoints, the now dominant revolutionary will enshrine their orthodoxy with the same tools and strategy that led to his or her preeminence. Having made a revolutionary advance, the dominant figure effectively prevents subsequent progress until surrendering or abandoning the field.

Sociologically, our thesis requires the community structure of science. In the absence of a research community able to react to the revolutionary's ideas, there will be nothing resembling scientific progress at all. One need only mention Leonardo de Vinci, Nickolas Copernicus, or Gregor Mendel to cite instances of revolutionaries who, because they worked in isolation, had their greatest effect upon science only when later researchers rediscovered their efforts (Leonardo, Mendel), or because their publications influenced a later generation (Copernicus) more than their own. In short, the revolutionary has the effect he or she has solely by virtue of his or her place in the group dynamics of a community. If not a member of a group, either in person, proxy, or by written communication, a revolutionary, (even a genius such as Leonardo) has no effect upon the intellectual scene at all.

Only relatively recently has the rhetorical dimension of science been elaborated: Krips (1994); Spence (1994); Fahnestock (1999); 1977
Simons (1980); Collins and Loftus (1995); Prelli (1989); Palmer (1981); Goss 1990) Weimer,W B (1977) There are
1990(1990

obvious factors in the argumentative claims of the novel thinker which are relevant. A revolutionary must be persuasive in making a case: he or she must understand their audience sufficiently to know which forms of argumentation will be successful in converting the community to the new viewpoint. Mere citation of data claims is hardly enough: one must argue for a particular conception of what 'data' is, and why it is or is not important in the given case.

This is why experimental results are often dismissed by revolutionaries. Likewise, appeal to logic (in either confirmation or refutation) is never enough. Quite often 'audience' is not at issue in a revolutionary reconceptualization, but only its conceptual significance. Here the revolutionary's skill in persuading the research community to see the world from his or her perspective must exceed the skill of the community in forcing its members to think in terms of already accepted paradigms (in both Kuhn's sociological and exemplary senses.) Were the revolutionary less persuasive than the audience, there could be no revolution at all. The novel thinker would either accede to the group, or leave the community.

Granted the success of a revolutionary, there is, however, an impellent restructuring of the permitted forms of communication and tuition within the community. The revolutionary, in virtue of his or her dominant position, will control the persuasive dimension of discourse—this theorist will tell the community not only what to think and do, but set the boundaries within which 'thinking and doing' can vary. The pattern of such theoretical claiming will be adjunctive: he or she will argue that 'since my theory is true, the world is *necessarily* so', and the normal science character of the community will be shaped by adherence to that claim. The revolutionary will even shape the nature of dissent from his or her views, because *dissent must presuppose the articulated framework of the revolutionary's position in order to be dissent from it.* Any revisionaries within the revolutionary's paradigm have to argue both *for* the revolutionary (on general matters) and *against* him or her (on detail matters), a task which requires enormous rhetorical skill to accomplish successfully.

For purposes of analysis this essay adopts a particular methodology that has been employed earlier by "author reference" for the understanding of science. Briefly, that methodology emphasizes that the nature of activity that constitutes science is qualitatively different both for two generic types of research ('normal' science versus 'extraordinary' or revolutionary) and also for levels of analysis at which it may be studied. The levels of analysis we focus upon are: within a theory, between theories in a research program, and beyond or behind theories ('paradigms' as metaphysical-sociological phenomena). Thus, this approach combines aspects of the positions of Popperian theorists (such as Lakatos) and Kuhn's conception of science, with various sociological, psychological, and rhetorical positions that are somewhat less familiar to philosophers. We rely upon and present examples of episodes that can be understood within that methodology. Our presentation both presupposes and also utilizes it in opposition to other methodologies in specific cases. In what follows Kuhn will not be taken as the last word, but at times he will be taken as the first word, upon which we build. However, it must be noted at the outset that this is a strategy to present our material rather than the only means to defend that view. No methodology yet available is adequate, and this essay presents data that all must either address or 'explain away'. Further, it must be stressed that while the phenomenon we document is characteristic of science it is not limited to science—rather, it seems to be an inevitable accompaniment of the psychological and sociological nature of the community structure of intellectual disciplines. We argue that it cannot therefore be ignored by methodologies of science (as a merely 'external' phenomenon) because its presence changes the cognitive content (or 'internal' nature) of the disciplines in which it occurs. Thus, to the extent that it occurs within science it must be acknowledged by analysis of scientific methodology.

What follows are four illustrations of our thesis. Although the personalities of Freud, Darwin Fisher, and Chomsky differ greatly, they each had the same constraining effect on the growth of their field.

SECTION 1

3.

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Representative Examples of our Thesis Regarding the Constraining Influence of the Revolutionary Thinker on the Growth of his Theory

SECTION 2

3.1. Classical Depth Psychology: Freud and the Development of Psychoanalysis

In 1913 Sigmund Freud secured the objectives of the Committee—a group of disciples dedicated to the preservation and defense of psychoanalytic precepts—by giving each member a Greek intaglio (traditionally used to seal contracts) from his personal collection. All six had the gift mounted on rings. Freud himself perceived the romantic element, and the experience was moving and profoundly symbolic for all present. With the formation of the Committee the future course of psychoanalysis was, in large measure, charted by personal commitments to one man.

SECTION 2

3.2. Freud's Character and the Purposes of the Committee

The goals of the Committee were specific. Jones (1955 a) recounts them:

> I proposed that…we form a small group of trustworthy analysts as a sort of "Old Guard" around Freud. It would give him the assurance that only a stable body of firm friends could, it would be a comfort in the event of further dissensions… There would be only one definite obligation undertaken among us; namely, that if anyone wished to depart from any of the fundamental tenets of psychoanalytic theory, e.g. the conception of repression, of the unconscious, of infantile sexuality, etc., he would promise not to do so publicly before first discussing his views with the rest….

The Committee undoubtedly fulfilled its primary function of fortifying Freud against the bitter attacks that were leveled against him. Freud's understanding of this inner council was in accord with that of its members. In his letter to Jones (1955 b) he wrote: "What took hold of my imagination immediately is your idea of a secret council composed of the best and most trustworthy among our men to take care of the further development of psycho-analysis and defend the cause against personalities and accidents when I am no more."

Freud here acknowledges his personal influence in directing the future course of analytic theory. His words also reveal that he was fully aware that Jones' primary motive in proposing the Committee was reactionary and designed to spare Freud the pain of further dissensions as had occurred with Jung, Adler[1] and Stekel in two previous years. The personal concern which was the foundation of the Committee was recognized by Freud, and the mystical-political overtones which enveloped the cause were certainly enhanced by Freud who wished that the Committee be a 'strictly secret' society. Grossworth (1992) adds that Freud displayed a fascination with secrecy since childhood.

The romanticism of his disciples was fired by this stipulation. Elements of idolatry and hero worship are readily found behind the general enthusiasm. Fanciful illusions to a sovereign surrounded by his champions are not entirely overdrawn: Freud's personality lent itself to this role. Physically he cut a handsome and imposing figure, a commanding image which was enhanced by his character. Characterologically he is described by Jones as at times austere, if not autocratic, yet always responsive and disdainful of all sham or hypocrisy; as affable and personable but also controlled, inaccessible, and secretive; as a man of integrity, courage, and deliberation, yet given to tale bearing and arbitrariness. Moreover, he was a gifted speaker whose lectures held his followers captivated for hours. Thus, there was much that was mysterious and enigmatic in Freud which could easily be perceived as awesome by those so inclined, and "uncanny" by several of his patients. This formidable personality exerted a dominating influence on the passions and ambitions of his adherents, an influence expressed for a time in unswerving devotion to Freud which exceeded commitment both to his theory and its proper inquiry. Indeed, such was the purpose of the Committee. And when so–called deviations from Freud occurred he was apprised of them by its other members **AQ9** .

SECTION 2

3.3. Freud as a Retarding Influence on the Growth of Psychoanalysis

Freud not only received but expected devotion from his followers. There is little question that Freud enjoyed the role of 'master' and was capable of exploiting the dedication and intense loyalty of students in the service of his wish to dominate. Early in his career Freud had already fancied himself as a bold, adventurous 'conquistador'. Certainly, Freud's predilection to play the 'hero' would elicit, if not require, 'hero worship' from his followers **AQ10** .

Nevertheless, a second streak in Freud's nature recoiled from such wishes. His aversion to all forms of sham, pretense, and hypocrisy, and his all but visceral contempt for displays of vanity or cant, extended into every corner of his life. He had greatest respect and preference for those about him, who were forthright and boldly original in their dealings with him. Jones (1955) describes this profound bent toward 'simplicity' as Freud's most distinctive characteristic **AQ11** .

It was quite probably the combined expression of these contradictory motives, serving first to encourage and then to inhibit free expression of new ideas among his followers, which is most responsible for the violent emotions that surfaced among all within the movement who eventually opposed Freud. Far more than theoretical differences, it was the personal reaction to the 'great man' which precipitated internal strife. For example, toward the end of their relationship Freud chided Jung for a slip of the pen. In an emotional outburst Jung (McGuire 1974) wrote Freud of his feelings toward him. Jung's remarks concerning Adler and Stekel (the first two defectors) are equally revealing:

> May I say a few words to you in earnest? I admit the ambivalence of my feelings towards you, but am inclined to take an honest and absolutely straightforward view of the situation. If you doubt my word, so much the worse for you. I would, however, point out that your technique of treating your pupils like patients is a blunder. In that way you produce either slavish sons or impudent puppies (Adler and Stekel and the whole insolent gang now throwing their weight about in Vienna). I am objective enough to see through your little trick. You go around sniffing out all the symptomatic actions in your vicinity thus reducing everyone to the level of sons anddaughters who blushingly admit the existence of their faults. Meantime you remain on top as nobody dares pluck the prophet by the beard and inquire for once what you would say to a patient with a tendency to analyze the analyst instead of himself.

Jung's angry remonstrations were clearly those of a man feeling demeaned and (in Freudian terms) infantilized, and who was seeking to regain a sense of autonomy. The correspondence of Adler and Stekel (as well as Rank and Ferenzci, whose defections in the early 1920's resulted in the dissolution of the Committee) is filled with similar recrimination towards Freud. Again and again personal ties to Freud dwarf all others in importance. Jones (ibid. b) refers to a letter written by Stekel, dated January 22, 1924. He paraphrases Stekel as saying 'things would have been different if only Freud had recognized in time that the pre-war dissensions had arisen from mutual jealousy in demands for his love rather than pretensions to his intellect **AQ12** .

Freud's patriarchal attitude towards his disciples was one which grew as the years passed. Freud the 'hero' and Freud the man dedicated to complete 'simplicity' combined to exact a steadily greater toll from his followers. Roazen (1975a , b) recounts how those surrounding Freud in the 1920's and early 1930's were passive listeners who never presumed to question the master-pupil relationship. The atmosphere was one of allegiance, submission, and intimidation. Freud's authority was unquestioned. Fears of exclusion from the movement with consequent loss of colleagues, literature citations, referrals and livelihood—as well as a place in history— intensified the dedication of Freud's widening circle. Freud the 'hero' had become a monarch **AQ13**

From this, recent biographers have drawn the most uncharitable if not acerbic denunciations. In Breger's (2009)

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judgment “theurpose of the Committee was to stifle debate and impose censorship,” Grosskurth’s assessment is similarly severe, impelling Phillips (1994) to characterize Grosskurth’s account as motivated by “snide zeal” “righteous indignation “and “Freud bashing.” Later she referred to the Committee as “a small group of henchmen”. More judicious accounts rely on the inevitable psychological and emotional consequences which we believe are commonplace in science. In this regard, Deutch (1973) writes:

> Gradually it came about that to many in this group the objective truth of Freud’s researches was of lessimportance than the gratification of the emotional need to be esteemed and appreciated by him. This emotional factor of subordinating one's intellectual freedom to the personal element became the source of the severest conflicts within the confines of this affect – laden circle. Each wished to be the favorite, and each demanded love and preference as compensation for having made the sacrifice of isolation.

And similarly, Kardiner (1997) tells us:

> He [Freud] was the dispenser of all the favors and patients for the entire group of analysts in Vienna, and this was the source of loyalty and corruption…. He had an enormous amount of control over both economic and status advancement. One should not fail to require that this was a corrupting influence, as it also created a great deal of rivalry, in-fighting, and maneuvering among his followers.

This account corroborates Jung’s observation that “Freud was placing personal authority about truth.” However, Jones (1959) writes: “That other part of Freud, which resisted all dissemblance, was contemptuous of those who had indulged his other half.” Helene Deutsch (1973) in a more measured statement of the same point writes: “It was never any fault of Freud’s that they cast him in this role and that they—so rumor has it—became mere yes-men. Quite the contrary; Freud had no love for them…And so it fell out that the very ones who proved to be the most loyal and the more reliable adherents were not the recipients of a warmer sympathy on his part."

Nevertheless, here as well, Freud's behavior still left room for further disagreement. For example: in a more tempered account, Jones (1955 b) "[Freud] was extremely patient and tolerant of divergent opinions so long as he thought the other person was sincere, and not merely impelled by irrelevant emotion." And Freud himself lends support to Deutch's assessments in a remark he made to his patient Eva Rosenfeld in the late 1920's: "The goody-goodies are no good and the naughty ones go away…" In his capacity as psychotherapist Freud's care and 'warmer sympathies' were more forthcoming, and it is altogether likely that those sycophantic followers who were denied Freud's respect as pupils would seek it (whether consciously or unconsciously) as his patients. Whenever they or those they cared about were in distress, Freud offered his services. Freud, the father figure, thus emerged as Freud the father confessor. And in this instance, dependency and infantilization was not only fostered, it was insured by the nature of the analytic process **AQ14** .

The two cardinal features of Freud's character thus played and interplayed; and in the final analysis they had a deleterious and restraining impact upon free exploration of his theory. Those within the Circle exposed to Freud's contradictory injunctions—to be intolerant of revisionism yet admiring of originality—and those who felt the pull toward submission yet were keenly aware of Freud's impatience and displeasure with any who remained too long under his tutelage, responded by becoming informed and discerning clarifiers and systematizers of Freud's own formulations. According to Roazen, (1955, pp. 301–302.) the highest compliment Freud could bestow during these years was that a pupil had given better expression to one of Freud's own ideas. "To reflect his own ideas back to him, without offering anything disturbingly new, was what he wanted from them. Freud's ideal sons and daughters (he greeted and addressed many of the men as 'my son') were embarked on insuring his scientific immortality." The man and his theory had become inseparable: 'personal disagreement represented scientific difference, and… scientific dispute constituted personal disloyalty.' To the majority of disciples this posed no dilemma. It is well known that Freud was protective of his discoveries and came to value them above personal friendships. But it must be realized that his students felt exactly the reverse. To them, Freud's friendship was far more important than their own ideas. Freud recognized the situation and three years prior to his death, at the 1936 meeting of the Vienna Psychoanalytic Society, Anna Freud conveyed a message from her father. In it Freud enjoined his followers not to identify psychoanalysis with him but to view it independently (Roazen 1975a , b). The near total obscurity of this event (it was conveyed to Roazen by Dr. Edward Kronold as a personal reminiscence) attests to the minimal effect it had on Freud's students, as well as the advancement of his theory *while he lived* **AQ15** .

Students in the 1920's sought Freud out less to learn than to serve. And Freud, now secure in his accomplishment—perceiving those about him as adding 'bricks' to the psychoanalytic edifice—had less incentive to encourage his more talented pupils to explore innovative lines of investigation. Rank and Ferenczi had defected from the Committee, leaving Ernest Jones, Hans Sachs, and Max Eitington (the sixth member, Karl Abraham, had died shortly earlier). As before, the more original (if at times undisciplined or intemperate) minds associated with Freud felt compelled to forsake him, while those remaining dedicated themselves to codification, clarification and organizational matters. As the thinking of Freud's disciples matured the same decision was forced upon each: to remain a loyalist and reject those ideas which deviated too strongly from orthodoxy, or to break with the movement entirely. A self-congratulatory Jones stresses how the Committee functioned perfectly for ten years (Sachs puts the figure at five) carrying out its purposes with remarkable harmony. Certainly, it cannot be denied that constructive ends were served by this cooperative effort. Nevertheless, what kept the members in harmony was a unitary policy, progressively inviolate, as to what was and was not suitably psychoanalytic. And this, in the case of Freud's most formidable colleagues, was felt as a paralyzing influence; one which was damaging both to their intellectual and personal lives. Even before the formation of the Committee, Jung had forebodings. In a letter dated January 3, 1913, Jung wrote to Freud. "It is my hope that the psychoanalytical movement will continue to advance, its vitality unimpaired and indeed heightened by internal conflicts and crosscurrents. Without them there is no life. When everything goes smoothly petrifaction sets in (McGuire 1974, 539)." His letter was written just prior to the final break with Freud.

In this light, the Committee was stultifying and provides not so much an explanation of why the movement ran so smoothly as why dissension and disaffection were so long in coming. Grinker (1973) concluded: "He [Freud] knew his name would be more revered after his death and the psychoanalytic movement furthered, for the figure of his person as the originator of psychoanalysis would be unavailable for attack." We would add that Freud's absence would allow those followers less disposed to his tenets to feel less resistance in supplanting them. Freud himself anticipated this. In a poignant letter to Ferenczi Freud wrote: "I have survived the Committee that was supposed to succeed me. I only hope that psychoanalysis will survive me." And surely it was *his* theory that he wished unchanged.

From the contemporary vantage, what was regarded as dissent, disloyalty, and rebelliousness by Freud and the orthodox center often looks more like minor squabbling. Ideas which were once the center of controversy are today so assimilated into mainstream thought as to draw no attention to themselves. And formerly heretical ideas such as the abandonment of the centrality of the oedipal complex, or Jung's rejection of the defensive structure of dreams, are no longer considered worthy of debate **AQ16** .

Contemporary analytical innovations, though they draw direction and inspiration from classical theory and indeed are prefigured in Freud's own writings, would unquestionably evoke his censure. Erikson's formulations tracing the development of ego processes are in one sense an inevitable outgrowth of basic psychoanalytic precepts, yet they remain thoroughly alien to the orthodox mind. The same can be said of early advances in 'ego psychology' beginning with the work of Heinz Hartmann and his notion of a conflict-free ego sphere. Although compatible with analytic theory they were decided departures from Freud's views. As a result, such developments as the very notion of psychic energy as the basis of drives came under question. Recognition of the role which social, cultural, and political forces play in personality development is of a degree which Freud would never have condoned. The same is true for the numerous schools of psychoanalysis which are viewed as extensions of the steadily evolving theory.

The same question arises with each advance: How is it that seemingly these inevitable and natural implications of Freud's theory could lay dormant and unrecognized so long? We argue that although they are thoroughly psychoanalytic, they were not Freudian. That is, although they accorded completely with the metaphysical-sociological 'paradigm' Freud set down; they did not conform to Freud's personal conception of what he had created. Today the only true connection remaining between psychoanalysis and Freud's research program is that none of the advances in psychoanalytic theory would have been conceivable without prior knowledge of Freud's accomplishments. But before his death Freud *was* psychoanalysis; and what he said was limited by the implications that *he* could entertain. This was the restraining order Freud's followers accepted until his death released them from it. Only then could Freud's ideas be studied independently of the man. Only after Freud died did Freudian theory take on a genuine life of its own.

Ultimately Freud's negative restraining influence fell *on himself* as well. Roazen (1975a , b) speculates that Freud never realized how much of a suggestive impact he had on his followers, and therefore could be led to think that his findings were being genuinely confirmed by independent observers. The reception Freud gave a student's work was often keenly felt. According to his disciple Herman Nunberg, a negative reaction frequently meant that a paper would not be pursued or submitted for publication. Thus, independence had been replaced by deference to authority. Thus authoritarianism, rather than ideas, guided the movement and, in the end, even Freud himself.

In her first analytic session with Freud, Choisy (1963) reveals that Freud said: "What will they do with my theory after my death. Will it resemble my basic thoughts?" On this point Sadger (2005), one of Freud's earliest and most devoted students remarked presciently: "When Freud departs from this earth, he leaves behind to be sure, disciples who continue to grow further, but no one to continue his teaching **AQ17** .

SECTION 2

3.4. The Committee and Contemporary History

If the aims of the committee seem remarkable it is not by virtue of being atypical. The history of the behavior of scientists bears testimony to the prevalence of such goals. What *is* extraordinary is that nowhere are such historical events discussed in standard histories. Certainly, this cannot be due to their obscurity. The history of the Committee, as well as the other episodes we discuss, are a matter of public record and readily accessible. Perhaps the ready availability of such information contributes, in part, to its neglect: what is common in history often is rewritten as commonplace. As such, personal motives and the force of individual personalities are discussed in biographical works (more likely by anecdote) but are too 'vulgar' to be accorded importance in the formal reconstruction of science. To the extent that "rational" or after the fact reconstruction is emphasized as indispensable such historical episodes will be wrongly minimized, impoverished and consigned to footnote or anecdote, and thus seriously neglected. Consider the Committee in this light: most histories of psychology ignore it and contemporary historiography does not accommodate it.

The exhaustive *History of Psychology* by G.S. Brett (1965) makes no mention of the Committee. Neither does Zilboorg's (1969) definitive *History of Medical Psychology*. In Ellenberger's (1970) scholarly evolutionary treatment of dynamic psychiatry and the notion of the unconscious, the Committee is conspicuous by its absence. While the existence of the Committee is duly noted in R.I. Watson's (1968) history, it is importance is not. In fact, Watson inaccurately recounts Freud's gift of the Greek intaglios in alleging that Freud presented each member with stones already mounted in rings. Such an error in an already abbreviated account assures the reader is denied any hint of the fervor which captured the emotions of the group.

While Sulloway (1979) makes no direct mention of the Committee, and regards Jones' biography of Freud as "monumental" he writes: "one must marvel at how remarkably good this biography really is." He then goes on to make two pertinent observations concerning Jones which are central to our thesis, and which reveal how spiteful one's loyalty can become:

> Jones saw himself in relation to Freud as T.H. Huxley – "Darwin's Bulldog"- had stood to the embattled Darwin a half century earlier…when writing his biography of Freud. Vessy-Wagner… particularly noted [Jones'] undiminished virulence toward all the old opponents of Freud and psychoanalysis. And when she "expressed doubts to Jones about the death of one individual: "Jones could scarcely conceal his pique when he wrote [to her] 'I don't care when he died so long as I can be sure he is thoroughly dead now, since I am libeling him severely.

Sixteen years after Freud's death, Heinz Hartmann (1955) confided to Ernest Jones that a genius could have an inhibiting influence on the men closest to him; and that he thought this was a central factor in the history of psychoanalysis.

SECTION 2

3.5. Evolution and the Neo-Darwinian Orthodoxy

One might argue that depth psychology is insufficiently scientific and therefore of limited concern. But no one could make a similar claim regarding evolution, and Darwin's role in its formulation.

Charles Darwin's role in the biological sciences (as well as in philosophy, physical sciences such as geology, psychology, and society at large) is so great that Loren Eisley (1958b) appropriately referred to the nineteenth century as Darwin's Century. There is no question that Darwin is one of the most revolutionary figures in the history of science—today that is taken for granted and research focuses upon attempting to specify what the nature of his 'revolution' was for a particular research community. The theory of evolution that Darwin promulgated, although fairly easily stated, had sufficiently broad and far reaching implications that its meaning varied considerably from context to context. And the fury of its defense (chiefly by champions of Darwin, like T. H. Huxley) brought a mastery of Victorian invective and personal zeal to the matter. Only a few of his scientific or clerical opponents recognized the gravity of the theory's shortcomings.

Eisley (1958a) writes: "The small brilliant band of men who by their united endeavor had swung world thought into a new channel had taken on something of the quality of myth, like the 'knights of the round table'." Acting distinctively like Freud's Committee,

> Darwin's champions: Huxley, Hooker, Galton, Spencer, Tyndall and others were members of the X-club; a monthly dining club of nine men who supported Darwin's theory and academic liberalism in the late nineteenth century. The author of the *Origin* was too worldly wise to think that could happen without help and…set about the task of surrounding himself with a coterie of serious scientists who were to be informed in advance of publication of the controversial ideas it contained. (Eiseley, 1958b).

Later, Eisley notes that Edmond Gosse (Gosse, 2009) in his autobiographical study *Father and Son* insinuated Charles Lyell's fundamental presence in this enterprise. "It was Lyell's conviction "that before the doctrine of natural selection was given to a world which would be sure to lift up at a howl of execration; a certain bodyguard of sound and experienced naturalists, expert in the description of species, should be privately made aware of its tenor. "Lyell's role as Darwin's singular protector on this matter parallels precisely the role played by Ernest Jones in the formation of Freud's Committee. In both instances the intent was to form a private, secret band of loyalists designed to shield the great man from critics, and thereby serve to obscure the doubtfulness of the tenability and utility of Darwin's theory.

The problems facing the theory of natural selection due to random variations were serious. Livio (1998) cites three:

> Lord Kelvin's calculations regarding the age of the earth (roughly one million years) did not allow for the far greater time required for natural selection to work. In the sixth edition of his *Origin of the Species* (2003*)* Darwin regarded the objection as "probably one of the gravest as yet advanced"; Darwin could adduce no credible reply (and it was forty years before Kelvin's objection fell.)
>
> Similarly, and in the same paragraph, Darwin confesses his vexation with the fossil record, namely "the absence of strata rich in fossils beneath the Cambrian formation."
>
> Yet surely the problem which most threatened to undermine the entire Darwinian edifice is what came to be known as The Swamping Problem, first elaborated by Fleeming Jenkin in the North British Review in June of 1867. It constituted a scathing attack on one of Darwin's most basic assumptions.

Hollman (1998), put the matter succinctly: 'Darwin assumed that the action of selection is slow. It was also commonly believed that under continued mating of the individuals within a species, the variants, whatever they may be, merely blended back into indeterminacy.' Darwin subscribed to this "blending hypothesis" even though it maintained that a variation in a single individual, no matter how adaptive it might be, was ultimately swamped (meaning diluted) during successive generations. Accordingly, natural selection on this view, did *not* favor some traits over others. Indeed blending was directly antithetical to change! Blending theory thus was untenable and incompatible with evolution. Darwin had been aware of the problem but minimized and dismissed it until 1867 when he read Jenkin's article, (1867) eight years after the publication of his *Origin*. In a letter to Wallace, Darwin confessed that Jenkin's arguments convinced him and he believed they could not be disputed! Darwin was left without a compelling alternative mechanism and ultimately adhered instead, along with the general scientific community, to an even less plausible variant of the blending hypothesis: Pangenesis **(**The view that reproductive cells contain gammules, invisible germs, from every cell in the organism, and are the bearers of heredity). The result was what Julian Huxley came to call the "Eclipse of Darwinism", accompanied by the inescapable retardation of the theory's growth. Consequently, Darwin desperately needed the X-club to secure his theory against rapidly arising alternative theories, which were seemingly more scientifically credible. Darwin the revolutionary was constrained by the reigning blending paradigm of his time which blinded him to his theory's most damaging anomalies. And he was ignorant as well of the redemptive powers of Mendelian genetics (Sclater, 2003) which provided a mathematically precise mechanism which *preserved* traits. Indeed, Darwin confessed in his autobiography (1958) that he found mathematics repugnant and thus while aware of Mendel never gave him serious consideration.

Remarkably Darwin found in his own experiments results similar to the ones Mendel discovered. He even came close to Mendel's 3:1 ratio. Yet, Darwin completely failed to grasp its critical importance in resolving the Swamping Problem. Moreover, as we shall see, Darwin surely had been acquainted with Mendel's experiments. Mendel's theorizing was able to and eventually did solve Darwin's swamping anomaly by supplying an adequate number of heritable variations. It took approximately a century, however, to integrate Darwinian selection and Mendelian genetics into what is now known as the Neo-Darwinism synthesis in which Mendelian genetics and Darwinian selection are thoroughly commensurable and mutually indispensable.

We now understand that while Mendel's work was (contrary to common misconception), *not* ignored so far as citations in the literature go (His work was quoted at least fourteen times before 1900, the year of his "rediscovery"); but it was ignored *by Darwin*. Garfield writes: "…Why did Darwin's 1876 paper cite Hoffman but not Mendel? Certainly, this is unusual, since Hoffman's paper cites Mendel five times." (1970) It was not so much Mendel himself but his discovery of laws of heredity which were ignored by most scientists. Certainly, Mendel's work was cited. But to be cited is far different from being accorded recognition and importance. And, Mendel's work went unrecognized for at least thirty-five years (and as many as seventy-two by estimates of other Darwinians), because Darwin did not bestow his personal imprimatur upon it. Lonnig (2003) puts the matter bluntly:

> All the evidence points to the main reason as follows: Mendel's ideas on heredity and evolution were diametrically opposed to those of Darwin and his Followers. Darwin believed in the inheritance of acquired characteristics…and, most important, of course, continuous evolution. Mendel, in contrast, rejected both the inheritance of acquired characters as well as evolution…as the Darwinians conceived it.

As Lonnig tells us it was pangenesis which won the battle for the minds in the nineteenth century: There was no space left in the next decades for the acceptance of true scientific laws of heredity discovered by Mendel…The general acceptance of Darwin's theory of evolution and his ideas regarding variation and the inheritance of acquired characters are, in fact, the main reasons for the neglect of Mendel's work.

Bowler (1986) writes: "[Darwin] realized that natural selection threw the emphasis onto those characters that were preserved intact by heredity; but he was incapable of visualizing those characters as units that could be transmitted independently of the process by which they were produced in the growing organism. On this view, it was Darwin's failure of imagination that prevented him from embracing Mendelian genetics and its redeeming implications for his theory." Bowler (1989) suggests that: "Indeed in his autobiography Darwin casts himself in virtually the same undiscerning light. Darwin writes: "I have no great quickness of apprehension or wit…and it is only after considerable reflection that I perceive the weak points. My power to follow a long and purely abstract train of thought is very limited."

Mendel's work appeared to Darwin as, at best, an adventurous anomaly; and it came to be the source of Darwin's most retarding influence. It is an example of what Frederich Waissmann (1968) meant when he said "the liberator of today may turn into the tyrant of tomorrow."

This presents a serious problem for the *internal* cognitive content of a discipline and *not* a mere external and irrational factor. In that regard, it was the purpose of the X-club to ensure that evolution *as Darwin conceived it* would survive intact. In a sense it might be argued that Darwin "led from behind", but his influence on the group is undeniable. Darwin's proponents protected Darwin, and more pointedly, the acceptance of his theory *without revision or change*, which perforce had a stultifying effect on its progress.

Darwin died with the Swamping Problem unsolved and its consequences haunted his last years. Indeed, Darwin wrote in his *Descent of Man* (1871), "I now admit…that in the earlier editions of my 'Origin of the Species' I perhaps attributed too much to the action of natural selection on the survival of the fittest." And Mayr, 1999 (1991) writes: "From about 1890 to 1910 Darwin's theory declined by such an extent by various opposing theories that it was in danger of being replaced." It is not generally recognized that Darwin was forced to withdraw the strong stand of the *Origin*, and Ward and Kirschvinck (2015) believe that Darwin's distress over the matter hastened his death. Yet the solution to Darwin's problem lay accessible in the publication of Gregor Mendel. Vorzimmer (1958) wrote: "Darwin was searching for the laws governing inheritance in the very year that Mendel was announcing his discovery of those same laws." Our argument concerns the timeline from Darwin's initial publication of the *Origin* in 1859 to a reasonably fleshed out neo-Darwinian explanation (roughly 100 years) beginning with "rediscovery" of Mendel's work in 1910. That is the year usually considered the time at which Darwin's theory could at last be placed on a "firm" genetic foundation.

We contend that powerful forces (some of them psychological) prevented Darwin from recognizing Mendel's work as of crucial importance to the survival of his theory, – and consequently retarded its growth. At the time that Darwin himself was very cautiously and indecisively wrestling with the problems of the emergence of man's brain (and the cranial capacity of the fossil record), he failed entirely to acknowledge the effect of culture (populations) upon man's development.

Twentieth century 'Darwinians' have had to reconstruct the original Darwinian natural selection notion, discard the excesses of his champions' and Darwin's own indecisiveness about man, revive genetics as an alternative to continuous blending, begin to acknowledge social factors and learning history of the organism, and more. Darwin's continued presence in addition to the persona presented to the public (Julian Huxley's remarks represent those of a third-generation champion) effectively retarded the field by presenting the conflicting position of brash but incorrect formulation next to cautious indecision. Livio (1998) concludes his remarks on Darwin with this observation:

> Darwin's blunder and Jenkin's criticism had an unexpected consequence. They essentially paved the way for the 'mathematical population genetics theory' developed by Ronald Fisher, J.B.S. Haldane and Sewell Wright." By dominating the development of statistics from 1915 to roughly 1955 R.A. Fisher contributed significantly to the dominance of nineteenth century Cambridge philosophy, with the result that his techniques determined the form of the majority of studies in psychological and sociological research into the 1970's.

Fisher's role in these developments reflect several of our themes. Often regarded as the "Father of inferential statistics," we next turn our attention to him.

SECTION 2

3.6. R. A. Fisher and the Social Sciences: Statistical Testing in the Social Sciences

Whereas the philosophy and methodology of scientific research deal with the conceptual problems of the nature of inference in science, the actual development of mathematical procedures to guide the design and conduct of experiments has always remained a separate discipline, more likely to be aligned with (and attract the talents of those congenial to) pure mathematics (dealing with the theory of distributions of populations and probability) rather than philosophy (confirmation theory and 'inductive' logic), even though both fields are quick to claim that they deal with the same problem: the assessment of the merit of scientific propositions. What is called inferential statistics is thus a matter of development of mathematical theory and technique (often of considerable sophistication) in the service of scientific methodology. From an external perspective the field of 'statistical design' presents a combination of features that are more or less out of phase with one another. As a matter of historical development statistical inference, theory and procedures have lagged far behind progress in the methodology of scientific research and, equally telling, theories of mensuration (Trendler, 2009). This results in the curious picture of a field which devotes increasing mathematical development to programs that stem from methodological perspectives that have been seen to be increasingly inadequate on philosophical and conceptual grounds. Philosophical naiveté (or single mindedness) on the part of some major figures has ensured that statistics teaches its consumers (chiefly the mathematically unskilled biological and social sciences, and also business and management programs) techniques that are often generations or more out of date. By dominating the development of statistics from 1915 to roughly 1955 R. A. Fisher contributed 'significantly' to the dominance of nineteenth century Cambridge philosophy in statistical methodology, with the result that his techniques determined the form of the majority of studies in psychological and sociological research even into the 1970's. Fisher's role in these developments reflects several of our themes.

The nineteenth century saw the quest for certainty in scientific knowledge, which had dominated thinking from the time of the Greeks, supplanted by gradual realization that the dream of Euclidean certainty could not be achieved. The transition in thinking from the methodologies of science proposed by J. S. Mill to those of William Whewell and W. S. Jevons indicates the shift from proof, certainty, and deductive inference to probability and induction as the essence of scientific statements. The alleged probabilistic structure of scientific knowledge now had to be mirrored in scientific method—thus a probabilistic inductive" logic" was necessitated to save the glory of science from being the scandal of philosophy, as in this oft quoted remark by C.D. Broad (1926): 'May we venture to hope that…Inductive Reasoning, which has long been the glory of Science, will have ceased to be the scandal of philosophy?'.

It is plausibly claimed that probabilistic inductive logic was a Cambridge invention, stemming from W.E. Johnson, developed by his and J.N. Keynes' students such as Broad and Fisher, which, following this general line of theorizing (Hogben, 1968) adopted the attitude of dogmatic falsificationism (Hollman, 1998), that although science could not prove a hypothesis to be true, it could (due to the asymmetry between *modus tollens* and the fallacy of affirming the antecedent) conclusively disprove one. Fisher's probabilistic reasoning came in the form of the 'logic' of null hypothesis testing. Assuming that science can be reconstructed as a forced choice between a null hypothesis (of no difference in parameters of different samples of populations) and an alternative (H_1 the experimenter's actual hypothesis under test) he reasoned that 'confirmation' (failure to reject H_0) left the truth value of the hypothesis obscured, while rejecting the null could be taken as confirmation of the alternative if the deviation from H_0 is 'significant.'

Hence Fisher regarded the significance test procedure qua mode of scientific inference as *the* general logic of experimentation, and *ipso facto* as a logic of induction or confirmation theory. Fisher writes: "That such a process (induction) existed and was possible to normal minds has been understood for centuries; it is only with the recent development of statistical science that an analytic account can now be given, about as satisfying and complete, at least, as that given traditionally of the deductive processes." (Fisher, 1957).

But despite Fisher's utilization of sampling procedures based upon the calculus of probabilities in his statistical tests his conception of scientific knowledge harks back to the classic form of justificationism rather than the probabilistic form of neo-justificationism (See: Weimer, W B (1979).

Fisherians regard the probability of a scientific hypothesis as either zero or one, as *absolutely* true or false, rather than as probable. The significance test is a statistical (probabilistic) procedure that is nonetheless supposed *to prove* a hypothesis (to be false).

Thus, the significance testing model of inference is actually of a form that was decisively criticized and abandoned by the philosophy and methodology of science well over a century ago. Yet despite this, Fisher managed to convey the impression to a majority of researchers in the social and biological sciences that his procedures exemplified the only acceptable forms of inference for all of science. While the field developed as a branch of mathematics Fisher nevertheless retarded its correct application in the social sciences; with broad influence as to what proper experimentation should be. Most conspicuously, the study of individual differences was all but proscribed. Case-specific and case control methods were rejected by Fisher, who knew little of them, yet opposed them because they did not rely *on randomization* which was the cornerstone of his theorizing. As Stagler (1999) writes: "Statistics and psychology have long enjoyed an unusually close relationship; indeed, more than just close for they are inexplicably bound together." And further (p.199) states: "…it creates and defines the objects of study."

As late as 1957 an exasperated but articulate rival (See: Hogben, 1957. Note 2)), defender of the Neyman-Pearson decision theory approach) began by acknowledging:

> That such a process (induction) existed and was possible to normal minds, has been understood for centuries; it is only with the recent development of statistical science that an analytic account can now be given, about as satisfying and complete, at least, as that given traditionally of the deductive processes (Fisher, 1957) [Fisher takes the] unusual precaution of collecting what he regards as his hitherto major contributions in an impressive quasi-memorial volume with a biographical introduction, with a portrait in of the author and with his own respectfully retrospective comments on the outstanding importance of each of the literary landmarks re-erected therein.

The remarkable extent of Fisher's dominance of statistical 'method' in the social sciences is attested by the numerous articles and collections of readings that still take significance testing for granted as a point of departure. A notable book of readings entitled *The Significance Test Controversy* (Morrison & Henkel, 1970) chronicles dissent within the ranks and some external criticism. Both authors of this essay recall being presented with Fisher's ideas during their graduate training as part of the folklore of science, as though it were common to all experimental research disciplines, with never so much as a hint as to the philosophy of science background or that it was the slightest bit controversial. Decision theory and Bayesian inference, when discussed, were presented as *additions* to significance testing rather than mortal enemies to it! While we admit that Popperian biases (with regard to the provability or 'probability' of hypotheses) may be showing through it seems to us that Fisher's successful influence, which hindered methodological advances in the 'soft' sciences for at least 80 years, is striking. One must inquire as to how he managed to do it.

Once more the picture of a revolutionary iconoclast, who single handedly articulated the first 'paradigm' of statistical inference procedures, emerges. Fisher was an institution in part because he totally supplied (and dominated) the reigning paradigm. His superbly skilled polemical/rhetorical writings made it appear that his arch rivals (such as Jerzy Neyman, father of decision theory with E. S. Pearson) were merely 'misinformed' commentators upon his views, and his corrections of their 'errors' set them back upon the only acceptable path, or made it clear that their views were relevant only to other areas (but not inferential statistics). Thus, decision theorists, who plausibly argued that 'inferential statistics' is a contradiction in terms, found themselves on the outside looking in before any debate began. Fisher took all controversy into his paradigm (thereby changing it drastically) simply because he would acknowledge no alternatives, ignoring reliance on his precursors. On this point Stigler (1999) regards Fisher's neglect of the seminal work of Francis Edgesworth as the most remarkable. As a result of this incorporation, there could be no progress for Statistical Methodology. As Neyman (Neyman et al. 1957, Note 45.) acknowledged retrospectively:

> Fisher was a fighter and, after reaching a result which satisfied him, he would struggle for the general acceptance of this result…. At times there were 'no holds barred' in the disputes that developed, and in the process, it was inevitable for Fisher to step heavily on the toes of some generally recognized contemporary authorities…. Another… spectacular occurrence was the December, 1934 meeting of the Royal Statistical

> Society. This was the first and, so far as we know, the only meeting of the Society at which Fisher was invited to present a paper (1935). The subsequent motion of the 'vote of thanks' (Bowley, et al., 1935) [the quotes are intentional and fitting in the situation] and the following discussion, all duly recorded in the Society's journal (1957). have few parallels in the scholarly literature known to us. However, the violent attacks Fisher sustained on this occasion were harmless. At the time Fisher had easy access to printing press, both through the *Annals of Eugenics*, of which he was editor, and by being a fellow of the Royal Society of London. With these advantages, Fisher could ignore the displeasure of the leaders of the Royal Statistical Society. Besides, the angry outbursts of several 'oldsters', countered by manifestations of Fisher's high polemical talent, impressed the audience as evidence of his originality. His following grew and the problems he was concerned with attracted more attention.

Moreover, Fisher's personality ("his psychology" if you will), undoubtedly fueled the man and intimidated his rivals in his attempts to powerfully bestride his field. In this regard, Stoley (1991), writes: "[Fisher's] zest for confrontation and polemic was legend; all who knew him comment on this, even his usually uncritical biographer/daughter. Domestic fits of temper paralleled the intolerance and aggressiveness he demonstrated toward his colleagues. He feuded with the two Pearsons, Karl and Egon; with Gossett; with Neyman; and with others. He hated to admit that he was wrong on any subject." Further Stoley, (1992 b) relates how Fisher even attempted to censor Neyman and force him to teach only Fisher's views! Neyman demurred, Fisher replied: "Well, if so, then from now on I shall oppose you in all my capacities," (which he proceeded to enumerate). Indeed, even Fisher's daughter and biographer, Joan Fisher Box (Box & Fisher, 1978) describes her father as even more acerbic and contentious. She writes: "He seemed inhuman sometimes in his lack of consideration for the feelings of others…. Capable of rough handling those who opposed him with ready-made arguments that he treated with contempt: He was sometimes arbitrary and disagreeable; and he was recalcitrant to any form of coercion." None of Fisher's detractors take issue with his brilliance. However, that brilliance and authoritarian personality engendered his dominance of the field. Characteristically he was a bully and he assuredly required no circle of defenders, as did Freud and Darwin; he was more than capable of defending himself. As a groundbreaking statistician he employed those traits to successfully champion himself while simultaneously constraining others from advancing the field in non-Fisherian ways. In a recent article Silberschatz (2015) demonstrates just how powerful these other largely neglected methods can be.

Fisher is rather unique in being a one-man paradigm (cum research program). Unlike Freud he neither needed nor would have tolerated a protective 'belt' of disciples. That research program was his alone, and it is not surprising that with his death that program ceased, and there are no Fisherians to be found, except in the applied fields that are awash with significance testers.

The continuation of Fisher's views is now dependent upon their 'common knowledge' or 'folk psychology' acceptance into decision theory and Bayesian inference procedures. Fisher's apocryphal presence is evident even in texts that attempt to damn everything he advocated (witness the first three chapters in Hogben, (1957, Note 2) being devoted to Fisher's 'pernicious' influence. Even mid-twentieth century methodological primers such as Turner (1965) that were avowedly Bayesian have their statistical sections shaped by their opposition to Fisher's procedures; and none of these psycho/sociological factors have found their way into Porter's (1986) otherwise excellent and synoptic account of the history of statistics.

SECTION 2

3.7. Chomsky and the Primacy of Syntax

Noam Chomsky occasioned one of the clearest recent cases of a scientific revolution (and the clearest that has yet occurred in the 'social' sciences), at about the time that Kuhn's reemphasis upon the phenomenon caused the 'crisis' in methodology. Beginning in obscurity in 1957 with a monograph, *Syntactic Structures* (1967) that was never intended for publication Chomsky proposed that the program of structural linguistics was explanatorily inadequate and should be replaced by a new approach which he called transformational (now also 'generative') grammar. As an outsider to mainstream linguistics Chomsky was fortunate to attract a powerful champion. Robert B. Lees, then a highly regarded young associate professor at Illinois (and a structuralist), reviewed Chomsky's monograph, became convinced of the utility and superiority of the upstart new approach, went to M.I.T. to study it further, and then published *The grammar of English Nominalizations* in 1960, the first championed defense of transformational grammar. Like Darwin's Huxley, Lees attended the majority of conferences, linguistic society meetings, etc., instead of Chomsky, and it is largely through his offices that Chomsky's views spread beyond M.I.T. (where he had the sanction of the powerful Roman Jakobson). Within psychology this bulldog role was taken up by polemically skilled M.I.T. philosopher Jerry Fodor. By the time of Chomsky's (1965) second major book the transformational revolution was largely *fait accompli* in linguistics, and was rapidly spreading into psycholinguistics and psychology.

What is even more noteworthy, in view of the time scale involved, is that by the early 1970s it had become obvious that a palace revolution had occurred, with the result that a near majority of transformationalists (especially those trained from the beginning on Chomsky's views) while clearly transformational generative grammarians in Kuhn's 'metaphysical paradigm' sense, had revolted against Chomsky's own research program. The particular form of Chomsky's linguistic theory was regarded as inadequate to its (self- appointed) task. Whereas Chomsky had successfully argued for the primacy of syntactic analysis over phonemics, the second-generation students of Chomsky opposed him by arguing for the primacy of semantics. The syntax/semantics controversy is a 'clear case' (like the famous 'colorless green ideas sleep furiously') of how the continued presence of a powerful revolutionary shapes the nature of his opposition and thereby retards developments that would otherwise occur.

Goldsmith and Huck (2000) argue persuasively that, in addition to Chomsky the man was his vital research program centered at MIT. It was the bastion of Chomsky's ideas with no less than 48 students publishing within his paradigm. No such center existed for the likes of semantics advocates such as Lakoff, Postal, McCawly and Ross, who were geographically dispersed with only five or so contributing students. Goldsmith and Huck continued "M.I.T's success in attracting bright students and rapidly turning them into productive members of the research program there is unparalleled among linguistics departments," and its students were expected to contribute research supporting Chomsky. While Goldsmith and Huck regard their contribution as "not inconsequential" we regard this self-generated research community as crucial in dominating the field with an imposing presence, and thereby restraining the advance of a viable generative semantics.

SECTION 2

3.8. Syntactic Structures and the Autonomy of Syntax

Chomsky began the first substantive chapter of *Syntactic Structures* by arguing that the study of language structure could be carried out independently of considerations of meaning. 'Despite the undeniable interest and importance of semantic and statistical studies of language, they appear to have no direct relevance to the problem of determining or characterizing the set of grammatical utterances. I think we are forced to conclude that grammar is autonomous and independent of meaning,' Chomsky, (1967).

Chomsky's argument is in a sense obviously circular: the traditional meaning of grammar has to do with form or syntax, and it is not surprising that if the fundamental aim in the linguistic analysis of a language L is to separate the grammatical sequences which are the sentences of language L from the ungrammatical sequences which are not sentences of L and to study the structure of the grammatical sequences (1967 b), then such study is 'autonomous' and independent of the meaning of those 'grammatical sequences.' At least a major part of Chomsky's motivation was to legitimate the study of syntax in opposition to phonemic entities (as the structural linguistics program had proposed).

The argument against meaning was also based on the famous 'Colorless green ideas sleep furiously', which is a 'clear case' of a semantically anomalous but grammatically well-formed utterance. Considering that, and clear cases such as 'the book seems interesting' versus 'the child seems sleeping', he concluded 'such examples suggest that any search for a semantically based definition of 'grammaticalness' will be futile' (Chomsky, 1967). Further, his structural analysis showed how such clear cases could be consistently explained by theoretically motivated principles rather than individual or ad hoc accounts such as traditionally were provided. Chapter 9 reformulated the issue clearly: The real question that should be asked is 'how are the syntactic devices available in a given language put to work in the actual use of this language' (1967). He answers thusly: 'Perhaps this problem can be elucidated somewhat further by a purely negative discussion of the possibility of finding semantic foundations for syntactic theory' And again he allows Goldsmith (1995): "It is possible that natural language has only syntax and pragmatics…There will be no provision for what Scott Soames calls 'the central semantic factor about language, … and that it is not assumed that language is used to represent the world…".

SECTION 2

3.9. 'Aspects' and Interpretive Semantics

Aspects of the theory of syntax (2015) elaborated the initial conception. After clearing up errors in the first grammar of *Syntactic Structures* (such as the 'Kernel /sentence confusion), Chomsky clearly detailed the idea that semantics is interpretive, that the semantic component is added to or operates upon the output of the purely syntactic base. This position was regarded as 'the standard theory', and later developed into 'the extended standard theory'. Not surprisingly, this was a period of turbulent development, and seemingly all issues were open to revision. As chapter 4, 'Some residual problems', states: 'we have been considering the possibility of assigning the function of selection rules to the semantic component. Alternatively, one might question whether the functions of the semantic component as described earlier should not be taken over, *in toto*, by the generative syntactic rules.' To this seemingly self-contradictory and radical possibility of syntactic 'theory' of semantics he only remarked, 'this notion, which is by no means to be ruled out a priori is explored by Bever and Rosenbaum (1971) who show that if it is adopted, the internal organization of the syntactic component must be revised in several essential ways.' Part of this openness was due to the first serious studies of semantics from the transformational perspective. The dictionary entry model of Katz and Fodor (1963) had broadened the nature of linguistic meaning, and was in the process of being shown to be inadequate, both by internal criticism, and devastating outside criticism such as Bollinger and Weinreich. Chomsky was occupied by numerous projects at this time, and serious study of semantic issues seemed to pass on to his students and associates for a period. This lessened the revolutionary's grip on the growth of his field. Indeed this was manifestly the case, as indicated by Randy Allen Harris' account (1993) of what happened at M.I.T during his two year sabbatical at Berkeley beginning in 1966. Before that Harris quotes Ross as calling that time Chomsky's "one man show." And Harris amplified the remark by adding "Chomsky sets the agenda whenever he is there, and setting the agenda at M.I.T in the first half of the sixties was tantamount to setting the agenda for transformational grammar." But "…almost everyone saw a significant correlation between Chomsky's 1966 sabbatical…and the dramatic rise in the fortunes of abstract syntax as it segued into generative semantics… with Chomsky gone their proposals became more daring, more original." To put a finer point on it, when Chomsky left, much of his retarding influence followed him.

SECTION 2

3.10. Language and Mind' as the Liar's Paradox

With *Language and Mind* (1968) and *New Horizons in the Study of Language and Mind* (1995), Chomsky had returned (or continued) to defense of interpretive semantics as the only appropriate way to go. This led to what, with hindsight, is paradoxical and the principal source of his retarding influence on the field. Previously his arguments for the autonomy of syntax had stemmed from conceptual considerations related solely to linguistics. In *Language and Mind* however, he admitted what was regarded as 'obvious' both to many of his students and to outsiders such as psychologists, referring to 'The particular branch of cognitive psychology known as linguistics.'

With the admission that linguistic analysis cannot be studied in isolation from psychology it becomes obvious that if Chomsky is right (about linguistics as a branch of cognitive psychology), then Chomsky is wrong (because semantics can no longer be interpretive). Chomsky's research program, although well motivated for destroying structural linguistics, had been abandoned by increasing numbers of students and associates beginning in the late 60's into the late 80's. The reason why is obvious—there is no principled boundary between syntax and semantics except from the standpoint of a 'grammatical' analysis. When one pursues a cognitive psychological analysis the primacy of semantic factors becomes obvious. The issue is not how meaning is manifested: Chomsky is quite correct that meaning is manifested through syntactic structuring. If one can understand that a 'venetian blind' is not a 'blind venation' that is no longer at issue. What *is* at issue is the ultimate nature of the entities which are so structured. Here the answer is more and more clearly that they are semantic and pragmatic, psychological and cognitive, rather than just linguistic and syntactic. The standard account, first as an 'extended standard account' (EST), and increasingly an 'extended extended' account, had slowly lost many of its original protagonists, and a welter of opposing positions, some of which had progressed enough in the late 80's to be correctly called research programs (albeit often no longer called generative semantics) had gained both 'old' converts and 'new' adherents.

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3.11. The Palace Revolution

The state of the art of the controversy was beautifully summarized and illuminated in *Semantics: An interdisciplinary reader edited by* Steinberg and Jacobowits (1971). The summary is an overview article by Howard Maclay, and the conflict is courtesy of Chomsky, McCawley, Lakoff, Fillmore, Katz, and others. Consider some of Maclay's comments:

> The key issue between Chomsky and the other linguists of [the third-generation formulation of grammar] is the autonomy of syntax. If no principled boundary can be drawn between these two areas, then there can be no distinct level of syntactic deep structure and the question of whether or not semantics is interpretive becomes irrelevant.
>
> It is fair to say that Chomsky now seems to occupy a minority position with regard to these issues. The representation of views in this anthology reflects, we think, the current distribution of opinion…. If this is the beginning of some sort of linguistic revolution, its magnitude should not be overestimated. The battle between Chomsky and his critics is being fought according to views which Chomsky himself developed and is essentially a sectarian war among scholars who share a common understanding as to the general goals of linguistic analysis…. This shared system of values permits confrontations of a very direct and intense sort among linguists who hold different views.

This description of a 'palace revolution' makes it clear that what was at issue were competing research programs within the transformational sociological-metaphysical paradigm. At issue is the best way to realize the promise of that 'paradigm', and the young revolutionaries, despite their other differences, all argue that Chomsky's particular program will not work. It cannot, without becoming as ad hoc and question begging as the earlier structural linguistic accounts.

Another factor that was involved during the period that the palace revolution was gaining strength was Chomsky's concern with extra linguistic topics. Just as he had earlier inadvertently aided the opposition by taking time out to do historical and phonemic studies, Chomsky increasingly took time out for social and political activism, an absence which enabled a lessening of loyalty to Chomsky by those with different ideas (elaborated in the section: Kuhn's community structure and the revolutionary.) As a result, the EST gradually became an increasingly defensive position, in that whereas Chomsky had initially pioneered the direction of research, effectively leading the field into new endeavors, the defenses of the 'received view' became exactly that—defenses. The direction of research switched to the 'young rebels' and Chomsky became increasingly occupied by *responding to* their views rather than forging ahead with his own program. His standard defense remains: that the proposed alternatives such as 'generative semantics' are in fact only 'notational variants' of the EST, and were thus mathematically equivalent in power to his formulation. While that may be true in the domain of algebraic structure theory and automata theory it was not seen as being relevant if linguistics is actually required to be compatible with cognitive psychology. One early leader of the generative semantics movement stated this implicitly at an interdisciplinary conference by presenting a paper titled 'Three batons for cognitive psychology' (Ross, 1974), which summarized three major problem areas in linguistics and then asked for help from psychologists both in explaining and finding other instances of the phenomena. There was no intention of surrendering linguistic inquiry to psychology, but rather a clear realization that the linguistic data were instances of cognitive (conceptual-communicational) processes rather than solely formal or syntactic ones.

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3.12. Chomsky as a retarding influence

Over time Chomsky's individual prominence in transformational linguistics slowly receded and in psycholinguistic and cognitive psychological research his views are in virtual eclipse. In linguistics, attention was focused then on at least four alternative research programs, and there followed an increasing number of attempts to move beyond the syntactic level of analysis with its conceptual primitive S (for sentence) to discourse analysis of (connected) sentences and a new, more psychological and semantic unit of analysis. Cognitive psychology has ceased to look to linguistics for models of meaning, and has instead developed abstract schema or prototype theories of meaning, within a framework of comprehension and communication of context dependent 'knowledge.' Pinker (1999) regards 'semantic stretch theory' and 'word structure theory' as the most regnant of present-day approaches. Those theories maintain that we create an infinitude of *meanings* in 'novel but appropriate' fashion just as the transformational approach once emphasized the same thing about *sentences.*

In a recent article it is clear that Ibbotson and Tomasello (2016) regard the 'palace revolution' as all but a *coup accompli*. They state "much of Noam Chomsky's revolution in linguistics—including its account of the way we learn languages—is being overturned." They take particular aim on Chomksy's "universal grammar theory" which they argue is being "abandoned…in droves because of new research examining many different languages…which fail to support Chomsky's assertions."

Chomsky and his colleagues have endeavored to preserve that theory by changing its definition. Chomsky's first version was one that was mathematically based in accord with the then current language of computer science, programmatic-sentence-forming procedures which were hard-wired and innate. However, multiple languages were found to be incompatible with that conception and Chomsky and his loyalists entirely replaced that approach with a set of 'universal' principles. However, non-European languages were also found to be incompatible with that approach, and that led Chomsky in 2000 to yet a third formulation with only one feature: computational recursion. Yet again, counterexamples undermined his third attempt. And a universal grammar, by definition, cannot entertain exceptions. Clearly Chomsky wishes to retain his bold theory of a universal grammar against the growing assault of empirical findings. Some of his followers have attempted to maintain their stance by introducing variables like immature memory, compromised attention, etc. which, they maintain, obscure the pure grammar beneath.

Such ad hoc measures are clearly *cognitive.* However, they are becoming necessary to contend with burgeoning anomalies in the field. And such attempts come to obstruct the development of better alternatives.

Citation of Chomsky and the EST is increasingly seen as a position to be summarized and then argued against. Indeed, Rudolph Botha entitled his 1989 text: "Challenging Chomsky". Chomsky is now an '*eminence grise'* haunting fields that must simultaneously praise him for the transformational approach and its benefits and then damn him for limiting its development. Analogously to Fisher in statistics even research that neither cites him nor mentions his name finds its problems shaped into their present form by his earlier domination and subsequent retardation of the field.

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3.13. The Insufficiency of Presumptive Methodologies to Explain Such Episodes

Popular methodologies can address the revolutionary's tendency to retard developments by remaining in the field only by attributing the effect to sociological factors that are more external than internal to the history of the discipline… This phenomenon is either not addressed (neither predicted to occur nor prohibited) or can be handled only by adducing *ad hocs* (some more plausible than others). Consider three examples that have been prominent since the middle of the twentieth century.

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SECTION 1

4. The Scope of the Problem

Other case histories could have been presented either in addition to or in place of ours outlined. For example, physics provides clear cases in Isaac Newton, Ernst Mach, Albert Einstein, and Niels Bohr. In economics J.M. Keynes is an obvious example (with the somewhat unique added effect of his martyrdom and premature fossilization due to an early death.) Philosophy provides numerous instances throughout its history (Karl Popper being an especially interesting example.)

What must be studied in all such cases is the manner in which any hard and fast internal versus external sorts of distinctions break down. Methodology must acknowledge that these cases reveal factors, (such as the temper of minds), that are part and parcel of the history of disciplinary inquiry instead of irrelevant or tangential matters that can be ignored.

Those acquainted with Max Planck's *Scientific Autobiography* may recognize this phenomenon. In it Planck relates a notably difficult episode in his career during which he attempted and failed to persuade the chemist Wilhelm Ostwald that the second law of thermodynamics could not be deduced from the first. "It is one of the most painful experiences of my entire scientific life that I have but seldom—in fact I might say never—succeeded in gaining universal recognition for a new result, the truth of which I could demonstrate by a conclusive, albeit only theoretical proof. All my sound arguments fell on deaf ears. It was simply impossible to be heard against the authority of men like Ostwald, Helm, and Mach.

In other disciplines similar phenomena occur. For example, the initial eclipsing of Mendel's work is little different from the early historical legacy of J.S. Bach. During the classical era Bach's works were known by composers. Beethoven revered him, and as a boy mastered Bach's entire Well-Tempered Clavier. Yet it is fully correct to say that Felix Mendelson did indeed 'rediscover' and restore Bach's importance and station in the musical canon with the recognition that comes only with public performances. Mendel and Bach similarly went from being "heard of" to being known as pivotal figures in their respective fields; and in that sense 'rediscovered'.

The effect of the "retarding influence" is surfacing in at least one other disparate, non-scientific domain as well. "Founder's Syndrome" refers to the behavior of certain founders of organizations whereby their initial conception eventually runs contrary to what its mission evolves to be. Thus, their zealousness restrains originality. The founder's original vision and inspiration comes to limit the organization's maturation and creative growth, because the founder retains decision making powers and consequently stultifies progress by constraining independent and innovative ideas.

Matters of length require that we restrict ourselves to traditional arguments in the philosophy of science and limit our consideration of current sociological trends. However, the sociological turn howsoever flushed out, must also address our thesis our thesis. And it too cannot, at present, do so.

SECTION 2

4.1. Kuhn's Community Structure and the Revolutionary

Kuhn's conception of scientific innovation as an essential tension between tradition and innovation does not address the time scale pertaining to the revolutionary's influence. Like most methodologies, his account plausibly groups secret societies, rings and disciples, and such sociological factors that could slow down progress, but it neither acknowledges this nor exploits it. One would assume that a revolutionary's continued presence should aid normal science development (by further exploring exemplary paradigms, etc.) in terms of internal (intellectual) history, so these cases in which the pervasive presence of a thinker's views stifles subsequent normal science growth are *prima facie* puzzling. If paradigms are the source of tacit knowledge that teaches novelty and extension to a new researcher it is hard to imagine that 'too much of a good thing' (continued exposure to a 'paradigm') could retard progress. Nothing in the disciplinary matrix or community structure of research that Kuhn emphasized seems to address the issue. The problem, of course, is that the effect is not *only* sociological but also psychological; a cognitive effect upon the research community; and that effect is ultimately an internal factor in the history of the field.

However, Kuhn does observe that the adherence of scientists to the old paradigm survives in the presence of the new, until the death or retirement of those followers; and that conversion to a new revolutionary theory is the province of younger scientists. However, what Kuhn fails to do is explain *why* the "old guard" is so tenacious.

It is our contention that this is the case not for reasons of dogmatism or logic but for *psychological* reasons which, perforce, range over qualities like loyalty, gratitude and love of the "great man," and attendant fear or guilt among adherents about replacing him, as well as the rhetorical power of his ideas. Those who knew him are respectful of his memory until he either leaves the field or dies.

This argument constitutes a considerable extension of Max Weber's notion of the 'charismatic authority', one of the three types of authority Weber considered legitimate. In Weber's (1978) view such authority rests "…on devotion to the specific and exceptional sanctity, heroism or exemplary character of an individual person and of the normative patterns of order revealed." Words like "sanctity", "devotion", and "heroism" begin to capture the psychology of the situation and the power of the constraints imposed by the progenitor of the theory on its progress. This aspect of the psychological and rhetorical situation does not lend itself to anything like an *ex post facto* logical reconstruction at all.

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4.2. Lakatos and the Methodology of Scientific Research Programs

At first blush the logical reconstruction of science proposed by Imre Lakatos (1970) seems more able to address the problem in a responsive way. His definitions of the 'hard core' and 'protective belt' constitutive of research programs might be read as an abstract paraphrase of, for example, Freud's Committee and its purposes:

> All scientific research programs may be characterized by their 'hard core.' The negative heuristic of the program forbids us to direct the *Modus Tollens* at this 'hard core'. Instead, we must use our ingenuity to articulate or even invent 'auxiliary hypotheses', which form a protective belt around this core, and we must redirect the *Modus Tollens* to these. It is this protective belt of auxiliary hypotheses which has to bear the brunt of tests and get adjusted and re-adjusted, or even completely replaced, to defend the thus-hardened core (1970).

Freud's Committee might be viewed, quite literally, as the *personification* of Lakatos' concepts. Recall Jones' description of the group of friends as an 'Old Guard', which would protect Freud and the tenets he held sacrosanct from internal dissension or external attack. As such the person of Freud becomes the living embodiment of the hard-core exacting, at least in the final analysis, undeviating loyalty to theoretical dogma (See: Brome (1969).

Conversely, the loyalty of membership served to insulate Freud (as with Chomsky and Darwin), manifesting itself as a living 'protective belt'. The followers advance the cause of analytic theory through limited investigation and theorizing which is open to debate and subject to continual revision and modification. It is the personal presence and ancillary work of Freud's disciples which bears the brunt of test and challenge from critics.

So, the analogy with Lakatos' position would run; but it need not be carried further. The reconstruction of science proposed by Lakatos is purely philosophical, specifically intended to be rational and *logical*, ranging over propositions and *not* personalities. Since he perceived himself as a Popperian, (despite being disavowed by Popper and his other followers) Lakatos was at pains to provide an alternative to Kuhn's '*psychology* of discovery' which Lakatos branded as mystical, religious, and thoroughly 'irrational'. Lakatos would have repudiated an extension of his views (by analogy or otherwise) which accommodated such factors. Any suggestion that the force of personalities or the attraction of individual charisma could rival that of reasoned discourse, or that allegiance to a man could surpass methodological commitments (for Lakatos, conventions) would be viewed as anathema to the rationality of science. The personal influence of the revolutionary thinker is of no internal historical consequence to Lakatos or orthodox Popperians. The felt presence of the 'great man' as it inclines his followers to work within the confines of his views, let alone to please him, indulge him, protect and ennoble him, cannot be recorded in the framework Lakatos provided.

SECTION 2

4.3. Polanyi and the Authoritarian Structure of Scientific Lore

Agassi (2014) writes, "No one wrote of leaders in science before [Polanyi] due to the popular prejudice that science is utterly rational and so free of leaders." Polanyi's account of the tacit dimension of science, in conjunction with the authoritarian structure imposed by the limits of personal experience of any researcher, presents a compelling picture of the extent to which one must submit to the ongoing structure of the scientific endeavor. But there is a two-way interaction in our submission to consensus:

"Submission to a consensus is always accompanied to some extent by the imposition of one's views on the consensus to which we submit…. Whenever I submit to a current consensus, I inevitable modify its teachings; for I submit to what I myself think it teaches and by joining the consensus on those terms I affect its content. On the other hand, even the sharpest dissent still operates by partial submission to an existing consensus: for the revolutionary must speak in terms that people can understand" (Polanyi, 2009). It is on point to mention in this regard that Freud was awarded the Goethe prize for literature.

While Polanyi's account is descriptively plausible it also says nothing about the temporal effects of submission to the consensus, or how the 'partial submission' of a revolutionary differs from other forms. Thus, without considerable development, it cannot address why a revolutionary's continued presence would retard progress, especially if the revolutionary is no longer contributing to the substantial development of that consensus. What is lacking is how continued submission to a consensus formed by an enduring authority figure can retard growth rather than foster successful rebellion leading to new growth. There is no doubt that the revolutionary's authority eventually suppressed dissent, but how and why Polanyi cannot say.

SECTION 2

4.4. Epistemological Naturalism and the Naturalistic/Sociological Turn

Given the thrust of our thesis one might correctly suppose that shortly after Kuhn's death the philosophy of science would change dramatically; and it did. Indeed, Kuhn's conception of the nature of scientific growth was virtually abandoned by most thinkers (especially after his turn to a "linguistics" account of science.) Kuhn's notion of incommensurability was deemed too imprecise, and rather than attempt to rectify it (as normal science practitioners would), the psychological model it implies was abandoned in favor of selecting only foreshortened and "sanitized" historical episodes for "rational reconstruction." With the exception of Kuhn's tacit acceptance of naturalism in the form of psychology and generational influences, Kuhn's work and his understanding of the growth of scientific knowledge was all but jettisoned by philosophers. In our estimation this change was not for the better and constitutes more a retreat from analytic philosophy to a position more akin to instrumentalism or pragmatism, and is certainly not an advance. The change is called epistemological naturalism. Ronald Giere (2005) provides a blunt synopsis of naturalism: He writes:

> Naturalism in the philosophy of science and philosophy generally, is more an overall approach to the subject than a set of specific doctrines. In philosophy it may be characterized only by the most general ontological and epistemological principles…. naturalists claim that all knowledge derives from human interactions with the natural world. This includes sense perception but may also include both techniques and technologies of human origin, such as statistical hypothesis testing and microscopes.

In brief naturalism constitutes a 'retreat to empiricism' albeit one of perhaps statistical character which derives from several sources: First, the recognition that attempts to ground knowledge upon an ultimate foundation have failed. Second, Quine's paper "Epistemology Naturalized" (1985) in which Quine argues for the replacement of epistemology by empirical psychology. And third, it is a retreat to instrumentalism a la Nagel of the 1950s. And we mentioned above that it even makes appeal to Kuhn's tacit positions on naturalism, with regard to his concepts of 'gestalt switches' and 'generational changes.' Certainly, our argument also employs psychological and sociological influences. However, naturalists regard those as *sufficien*t for an epistemology, as a replacement for philosophy.

It is not unusual for formerly philosophical problems to be resolved by science; however, naturalism would have it so for *all* its problems. As such naturalism entails the abandonment of a theoretical understanding of scientific growth and an abdication to empirical science itself, which now holds dominion as regards the best source of knowledge. Some naturalists ally themselves with materialism, others with pragmatism, and some with evolutionary epistemology.

The naturalist turn has embraced what has come to be known as the sociology of knowledge, or more accurately, of *belief.* In its most extreme, radical sociologists believe that all of science, and its traditional methodologies, is completely married to social forces. Nola (2008) writes:

> Social studies of science have not been content with merely investigating the external socio-historical context of science but have also looked at the very claims made within science itself. The difference here is a distinction philosophers make and sociologists often ignore, viz., between belief which is a naturalistic notion, and knowledge or rational belief, which are normative notions in that they involve rationality conditions such as reasons and justifications for belief, or coherence of belief.

We have no quarrel with the conjecture that social norms and constraints influence a theorist's beliefs. And our thesis is thoroughly compatible within an evolutionary epistemological framework. However, any viable naturalism would have to address issues such as the retarding influence of the revolutionary as well as their 'psychology.' (We believe the disciplinary divide between sociology and psychology is unnecessary and detrimental in this regard.) Consequently, these matters remain unsettled and unexplored within that framework. With regard to our thesis, we maintain that it is one to be reckoned with in *any* account of the growth of knowledge.

SECTION 1

5. Broader Implications of Our Thesis

For Kuhn, scientific revolutions (a change in a tradition of normal science puzzle solving) occurs when anomalies subvert the existing tradition. We regard Kuhn's account as insufficient since it postulates nothing as to when and how that determination is made or by whom. Anomalies are not in themselves causative of revolutions. That requires an assessment and determination by scientists as to their gravity, their insurmountability within the previous paradigm, and the persuasiveness of the revolutionary; all of which are psychological matters. One manner in which our thesis may be restated is: scientific revolutions are as much about the revolutionary as they are in Kuhns purely analytical account.

It may well be that in addition to the revolutionary's rhetorical gifts, the revolutionary's theory is received by many normal science practitioners, who after decades or even centuries of failed attempts to resolve anomalies leave many of them discouged, dispirited. and welcoming of a new paradigm with its promise of stimulating discoveries, and new research programs. In this respect we offer our work as a prolegomenon to psychological aspects of the scientific enterprise. With the possible exception of Darwin, all the revolutionaries we explore share two notable psychological traits: a prominent self-confidence (at times overweening, i.e. Fisher) and a propensity and gift for self-promotion. It should not be surprising that psychological factors play an important role in the growth of knowledge; science being a singularly human undertaking.

Additionally, psychological markers as to *when* a revolution not as yet a tradition is said to occur, is suggested in some of the cases we describe. For Freud, it is when he begins to spend time correcting his adherents and rallying the troops to his side and discontinuing his own "new" research. For Fisher, it arrived when he began polemicizing against alternatives.

Our case illustrations allude, as well, to the fact that the revolutions do not exclude parallel developments. For example, Fisher's domination of his field did not preclude others like Neyman and Pearson from their work advancing the field of statistics. Both groups contributed to what was to become a new tradition. Kuhn addresses this matter more broadly from an evolutionary point of view as the "essential tension" between tradition and innovation. It occurs in all research traditions not solely the ones we focus on.

Once the role of psychology is admitted, the idea of a thoroughly rational reconstruction of science becomes untenable and an account of the growth of knowledge more difficult. All that can be said on this point is that history is an empirical matter and cannot be given a definitive a priori explanation in historicist terms.

SECTION 1

6. Broader Implications of Our Thesis

Our essay contains empirical, psychological and philosophical components. The empirical component is the demonstration of episodes in the history of science which show that the continued presence of a revolutionary figure does in fact retard the progress of his or her field. Philosophically it shows that the traditional approach of after-the-fact rational *re*construction cannot address or explain such instances. Such approaches must ignore them, or explain them away as not either part of science, or the acquisition of knowledge. This is a defect of the purely philosophical theories of the growth of knowledge. Psychologically the essay points to the necessity of incorporating individual psychological factors in the explanation of *both* the revolutionary figure's behavior *and* also the individuals who make up the community structure of science. In this regard available sociological accounts (as represented by Kuhn), even though they refute the purely philosophical accounts proposed by residual logical empiricists and also their Popperian and more recent critics, are likewise inadequate. Such accounts must be supplemented by serious study of individual psychological factors. Consider this last point in more detail.

For the historian and sociologist Kuhn, scientific revolutions (a drastic change in a tradition of normal science puzzle solving) occurs when anomalies subvert the existing tradition. We regard Kuhn's account as insufficient since it says nothing about when and how that determination of inadequacy and potential replacement is made, or by whom. Anomalies are not in themselves causative of revolutions. That requires an assessment and determination by the community of scientists as to their gravity and insurmountability within the previous paradigm, as well as the persuasiveness of the revolutionary (and his or her champions). All of these are psychological and epistemological matters. One manner in which our thesis may be restated as this: scientific revolutions are as much about the revolutionary individual as they are about Kuhn's purely sociological analysis of community structure.

It may well be that in addition to the revolutionary's rhetorical gifts, the revolutionary's theory will be welcomed by many normal science practitioners who, after decades or even centuries of failed attempts to resolve what they increasingly perceived to be anomalies, have left many of them discouraged, dispirited, and thus welcoming of a new paradigm with its promise of stimulating discoveries and new research. The present essay is a prolegomenon to the study of psychological aspects of the scientific enterprise in order to assess such factors. With the possible exception of Darwin, all the revolutionaries we discussed share two notable psychological traits: very prominent self-confidence (at times overweening, i.e., Fisher), and a propensity toward and gift for self-promotion. It should not be surprising that such psychological factors play an important role in the growth of knowledge in addition to sociological ones. We cannot forget that science is a singularly human undertaking, and that humans occur both individually and in groups.

Additionally, some psychological markers are suggested as to when a revolution which has not yet become a normal research tradition. For Freud, it becomes clear when he begins to spend time correcting his adherents and rallying the troops to his side, and in so doing discontinues his own "new" research. For Fisher, it arrived when he began polemicizing against alternatives, and thus ignored his own continuing research direction.

Our cases allude, as well, to the fact that the revolutions do not exclude parallel developments of alternative research programs. For example, Fisher's domination of his field did not preclude others like Neyman and Pearson and Thomas Bayes. from their work advancing the field of non-Fisherian statistics. Both groups contributed to what was to become a new tradition. Kuhn does address this matter more broadly from an evolutionary point of view as the "essential tension" between tradition and innovation. It occurs in all research traditions, not solely the ones we focus upon. But the factors underlying this tension are ultimately psychological, effected by individuals within the community structure.

Once the role of psychology is admitted, the idea of an explicitly rational and thus purely philosophical reconstruction of science becomes untenable, and thus an account of the growth of knowledge much more difficult. All we can say at this point is that history remains an empirical matter, and cannot be given a definitive a priori explanation in any sort of historicist dialectical or explicit philosophical terms.

SECTION 1

7. Summary

Our examples document the pervasive presence of a phenomenon within the community structure of intellectual research groups, such as science, the social studies, and philosophy: how the enduring presence of a once revolutionary figure constrains the possibilities for development within his or her community, and thus stifles or retards growth (both developments within their research program, and revolutionary innovation).

This effect is an *internal* factor in the history of a discipline, if for no other reason than because knowledge is a rhetorically shaped product that results from the interaction of a researcher within his or her community. Thus, it follows that a revolutionary figure constrains possible developments by, for example: limiting their involvement in the dialogue (written and other formulations of it, and in general) by limiting all communication to what is compatible with their 'commanding presence' (at least as determined by the relevant community's perspective). Conversely, when a revolutionary abandons a field soon after an innovative contribution, the way is clearer for the community to develop views within the general 'paradigm,' which in effect proliferate rival research programs that the revolutionary might have suppressed. Such 'abandonment' cases present rapid development of 'superficial' alternatives, but little in depth exploration of the revolutionary's initial research program.

Examination of representative methodologies of science reveals that few can address these phenomena at all. Philosophical accounts, which concentrate upon rational-reconstruction of 'internal' factors, have failed to acknowledge it, and argue that they must be 'external' and irrational rather than internal. More sociologically oriented accounts can often plausibly make room for the phenomena, but there is no internal motivation to do so, and one or another ad hoc arguments (some more plausible than others) must be invoked to accommodate them. It would appear that only accounts acknowledging the rhetorical nature of argumentative claiming within research communities as well as multiple levels and types of scientific activity can plausibly address these instances at present. In that framework one can at least begin to understand why even the revolutionary's mortal revisionary enemies grudgingly concede the measure of their opponent, (but "Brutus is an honorable man.").

If the nature of knowledge is a product of psychodynamic factors and argumentative interchange, then they must indeed 'Give the devil his due' in order to successfully rebel against the revolutionary. Thus, this essay should be understood as a plea to expand the growing psychological and sociological analyses to include a rhetorical component as well; and a caution that such accounts must then be assayed against philosophical and historical ones, as well as the greatly expanding historical record itself.

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1 Our thesis is sharpened by the fact that when Adler defected and established his “Society for Individual Psychology” nothing could be said against it (Sadger, 2005). Similarly Makasi (2008) says the same of Jung. Those with their new-founded communities were “increasingly defined by the authority of a charismatic leader.”.