THE CONTINENTAL DRIFT CONTROVERSY

Volume IV: Evolution into Plate Tectonics

Resolution of the sixty-year debate over continental drift, culminating in the triumph of plate tectonics, changed the very fabric of Earth science. Plate tectonics can be considered alongside the theories of evolution in the life sciences and of quantum mechanics in physics in terms of its fundamental importance to our scientific understanding of the world. This four-volume treatise on The Continental Drift Controversy is the first complete history of the origin, debate, and gradual acceptance of this revolutionary explanation of the structure and motion of the Earth’s outer surface. Based on extensive interviews, archival papers, and original works, Frankel weaves together the lives and work of the scientists involved, producing an accessible narrative for scientists and non-scientists alike.

Explanations of the curious magnetic anomalies on the seafloor and discovery and explanation of transform faults in the ocean crust in the mid-1960s led to the rapid acceptance of seafloor spreading. The birth of plate tectonics followed soon after with the geometrification of geology. Finally it was understood that the Earth’s surface is divided into a small number of nearly rigid plates, most of which contain continental and oceanic parts, and whose relative motions are describable in terms of Euler’s fixed point theorem. Although plate tectonics did not explain the cause or dynamic mechanism of drifting continents, it provided a convincing kinematic explanation that continues to inspire geodynamic research to the present day.

Other volumes in The Continental Drift Controversy:
Volume I – Wegener and the Early Debate
Volume II – Paleomagnetism and Confirmation of Drift
Volume III – Introduction of Seafloor Spreading

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"What is so impressive about this monumental work is its completeness. Frankel has gone back to the original sources and papers, to ensure complete understanding of the scientific issues involved. I recommend these volumes to anyone interested in the subject."

DAN MCKENZIE, University of Cambridge

"Detailed and painstakingly researched, this account is the culmination of the author’s research into this topic over more than thirty years. It is difficult to imagine a more comprehensive analysis of the relevant literature and of the attitudes of the scientists involved."

FRED VINE, University of East Anglia (Emeritus Professor)

"Frankel has performed a huge and very valuable effort in pulling together not only original published sources but also unpublished correspondence from key authors, creating a timely and consistent picture of the crucial period when this revolution in the earth sciences occurred."

LYNN R. SYKES, Lamont-Doherty Earth Observatory, Columbia University
## Contents

1. Foreword by Steven Cande  
   1.1 Introduction  
   1.2 Wilson, the man  
   1.3 Wilson champions contractionism and continental accretion, 1949–1954  
   1.4 Wilson and Scheidegger raise difficulties with continental drift and mantle convection as the cause of island and mountain arcs, 1949–1954  
   1.5 Wilson continues to support contractionism and reject mobilism, 1959  
   1.6 Scheidegger acknowledges paleomagnetic support for mobilism, 1958, 1963  
   1.7 Wilson combines slow Earth expansion and his contractionist account of orogenic belts, 1960  
   1.8 Bernal and Dietz discuss seafloor spreading, October 1961  
   1.9 Wilson becomes a mobilist, 1961  
   1.10 Wilson matches the Cabot and Great Glen faults in support of mobilism, 1962  
   1.11 Menard and Hess correspond about seafloor spreading, 1961, 1962  
   1.12 Menard defends seafloor thinning and attacks Earth expansion, 1962  
   1.13 Heezen attacks seafloor spreading and seafloor thinning, 1962  
   1.14 Irving cautions Hess, 1961  
2. The origin of marine magnetic anomalies, 1958–1963  
   2.1 Introduction  
   2.2 Explaining marine magnetic anomalies
2.3 Interpretation of northeast Pacific marine magnetic anomalies prior to Vine’s proposal, 1958–1962 65
2.4 Interpretation of magnetic anomalies over ridges and seamounts prior to Vine’s proposal, 1953–1962 70
2.5 Matthews, his early life; goes to Antarctica 84
2.6 Matthews visits the Falkland Islands and favors continental drift 87
2.7 Matthews, his graduate work in the Department of Geodesy and Geophysics at Cambridge, 1958–1961 89
2.8 Vine’s early interest in continental drift and undergraduate years at Cambridge, 1959–1962 91
2.9 Vine begins research, 1962 96
2.10 Mason and Raff on magnetic anomalies, 1962–1963 101
2.11 Vine reviews the literature on marine magnetic anomalies, October 1962 to January 1963 104
2.12 Matthews’ meticulous survey over the Carlsberg Ridge, November 1962 109
2.13 Vine develops the Vine–Matthews hypothesis, early 1963 114
2.14 Morley’s education and early work in paleomagnetism and aeromagnetic surveying 124
2.15 Morley accepts reversals of the geomagnetic field and continental drift 127
2.16 Morley’s hypothesis 130
2.17 Morley’s paper is twice rejected 136
2.18 Why Morley’s paper was rejected and Vine and Matthews’ paper was accepted 139
2.19 Two other Vine–Matthews-like hypotheses 141

3 Disagreements over continental drift, ocean floor evolution, and mantle convection continue, 1963–1965 148
3.1 Introduction 148
3.2 Wilson continues seeking further support for mobilism 148
3.3 The Royal Society’s 1964 symposium on continental drift 162
3.4 The Everett, Bullard, and Smith fit of the continents surrounding the Atlantic 170
3.5 Paleomagnetism, other new evidence for continental drift, and mobilism’s mechanism difficulty 186
3.6 Menard ends his flirtation with mobilism 193
3.7 Menard attacks seafloor spreading and Wilson’s work on oceanic islands 198
3.8 Early responses to the Vine–Matthews hypothesis, 1964 202
3.9 Holmes on mantle convection, seafloor spreading, and Earth expansion 216
3.10 Rapid Earth expansion under attack, 1963–1964 227
4 Further work on the Vine–Matthews hypothesis and development of the idea of transform faults, 1964–1965 233
  4.1 Introduction 233
  4.2 Initial difficulties facing the Vine–Matthews hypothesis 234
  4.3 Vine, Matthews, and Cann defend and further develop the Vine–Matthews hypothesis, June 1964 to May 1965 236
  4.4 Mild support and criticism of the Vine–Matthews hypothesis during the first half of 1965 241
  4.5 Heirtzler, Le Pichon, and Talwani at Lamont Geological Observatory 245
  4.6 Lamont’s view of mid-ocean ridges and rejection of the Vine–Matthews hypothesis: work on the Mid-Atlantic Ridge 248
  4.7 Vine, Wilson, and Hess at Madingly Rise, late 1964 to middle 1965 255
  4.8 Hess fine tunes and extends seafloor spreading, 1965 259
  4.9 Wilson develops the idea of transform faults 261
  4.10 Wilson’s third trip around the world 269
  4.11 Vine independently proposes ridge-ridge transform faults 278
  4.12 Alan Coode’s idea of transform faults 280

5 Continuing disagreements over the Vine–Matthews hypothesis, transform faults, and seafloor evolution, 1965 293
  5.1 Outline 293
  5.2 Wilson and Vine work in the northeast Pacific 294
  5.3 Lamont’s view of mid-ocean ridges: work in the northeast Pacific 304
  5.4 Lamont’s view of mid-ocean ridges: work on the Reykjanes Ridge 310
  5.5 Geomagnetic reversals, the dominance of remanence, and Vine’s Ph.D. dissertation, August 1965 317
  5.6 Matthews seeks to explain the greater amplitude of the central anomaly 323
  5.7 The Ottawa meeting, September 1965 325
  5.8 The challenges of unraveling the Cenozoic history of the northeast Pacific 338

6 Resolution of the continental drift controversy 340
  6.1 Outline 340
  6.2 Lamont workers argue distribution of Atlantic Ocean floor sediments is incompatible with seafloor spreading: Hess disagrees 341
  6.3 Vine learns of corrections to the reversal timescale and fully accepts seafloor spreading: the November 1965 GSA meeting 345
  6.4 Improvements in the reversal timescale during 1966 350
  6.5 Opdyke and others at Lamont develop a reversal timescale based on the study of deep-sea cores 359
  6.6 Pitman’s “magic” profile over the Pacific–Antarctic Ridge, December 1965: Pitman, Heirtzler, and Talwani accept V-M 363
6.7 Cox and Doell become mobilists 374
6.8 Vine turns the Vine–Matthews hypothesis into a difficulty-free solution 375
6.9 Sykes confirms ridge-ridge transform faults 386
6.10 Menard accepts seafloor spreading; Heezen renounces rapid Earth expansion 403
6.11 Lamont workers argue that heat flow over the Mid-Atlantic Ridge is too low for seafloor spreading; Hess disagrees 405
6.12 The Goddard conference: selling continental drift and seafloor spreading to the establishment 412
6.13 Making sense of why Le Pichon, Heirtzler, and Talwani tried so hard to prove Hess wrong 418
6.14 Maurice Ewing reluctantly accepts discontinuous seafloor spreading 428
6.15 Why seafloor spreading was rapidly accepted by most marine geologists and geophysicists 431

7 The birth of plate tectonics 437
7.1 Outline 437
7.2 Bryan Isacks and Jack Oliver at Lamont 438
7.3 Isacks and Oliver launch their study of deep earthquakes 443
7.4 Isacks and Oliver pin down subduction 446
7.5 McKenzie, the making of a geophysicist 456
7.6 McKenzie interprets heat flow data in terms of seafloor spreading 463
7.7 Heirtzler and company extend the reversal timescale 469
7.8 Morgan discovers plate tectonics 474
7.9 Morgan’s presentations of plate tectonics: from April 1967 talk to March 1968 publication 478
7.10 Morgan’s April 1967 AGU presentation 481
7.11 Morgan’s 1968 paper “Rises, trenches, great faults, and crustal blocks” 485
7.12 McKenzie discovers plate tectonics 494
7.13 The keys to McKenzie’s discovery 510
7.14 McKenzie and Parker’s version of plate tectonics 516
7.15 Comparison of Morgan’s and McKenzie and Parker’s presentations of plate tectonics 527
7.16 Isacks spearheads discovery of how deep earthquakes beneath island arcs are caused 539
7.17 Le Pichon loops plate tectonics around the world 552
7.18 Isacks, Oliver, and Sykes integrate seismology and plate tectonics 562
7.19 Atwater and Menard apply plate tectonics to the great fracture zones of the northeast Pacific 576
Contents

7.20 The Great Magnetic Bight explained in terms of seafloor spreading 584
7.21 McKenzie and Morgan explore the evolution of triple junctions 591
7.22 Towards the new paleogeography: the complementarity of plate tectonics and APW paths and the triumph of mobilism 604

References 617
Index 656
Graduating from Yale in June of 1970 with a major in geology and geophysics I was at a loss as to what to do with the rest of my life. It was the height of the Vietnam War, the previous months had seen disturbing anti-war protests in New Haven and around the country, and I was disillusioned with academics. I had switched majors from engineering to geology and geophysics at the start of my junior year not because of any particular interest in geology, I had not yet taken any geology courses, but because I liked the atmosphere in the geology building where I had a bursary job in my sophomore year polishing meteorites. I quickly learned that there was an ongoing revolution in the earth sciences involving continental drift and seafloor spreading. There were talks by J. Tuzo Wilson, complete with paper cut-outs showing how transform faults work, and by Teddy Bullard on the remarkable fit of the continents around the Atlantic Ocean. I read Arthur Holmes’ classic textbook on physical geology which had recently been updated with a long section on paleomagnetism and continental drift. I found a discarded plot showing the location of earthquake epicenters around the globe which I scotch taped to my dorm room wall. However, my senior project involved working on a camera to take cross-sectional photos of fecal pellets on the floor of Cape Cod Bay and by the time I graduated my interest in geology was waning.

A serendipitous encounter at the end of the summer with Jim Walker, one of my undergraduate mentors (the conversation went something along the lines of “Steve, scientists at Lamont are often looking to hire recent grads to go out to sea”) led to an interview the next day with Walter Pitman, who was then the head of the marine magnetics group, and a job offer. Walter gave me some papers to read about plate tectonics and suggested that I try to use the shape of marine magnetic anomalies to determine where the Pacific plate had moved over the last 80 million years. He soon sent me out to sea on the R/V Conrad for six months to look after the magnetometer and digitize the magnetic records. I have been working with marine magnetic anomalies ever since.

Upon arriving at Lamont I discovered that there had indeed been a revolution in the earth sciences and that many of the Lamont scientists, including Walter, had
played pivotal roles. Walter was famous for his “Eltanin-19” profile, a magnetic anomaly record collected on the USNS *Eltanin* across the Pacific-Antarctic ridge and published in 1966 which displayed stunning mirror image symmetry about the spreading axis. Pitman’s “magic profile” was a tipping point in the acceptance of seafloor spreading and continental drift by the global earth science community. I soon learned that the number of people at Lamont who were involved with the plate tectonic revolution was not only large but consisted of a fascinating coterie of characters. There was a seemingly endless set of stories about what had happened at Lamont during the heady days of the revolution, who had had flashes of brilliance, who had missed the obvious, and who had been slow to come around to accepting seafloor spreading.

At the top was Doc Ewing, the founder and director of Lamont, who was a “fixist,” which meant that he was opposed to the revolution, but whose vision for collecting all types of geophysical data on two ships constantly circuiting the globe, even data whose usefulness was unclear, eventually provided Lamont scientists with an unparalleled global data base to exploit the implications of plate tectonics. He ran a tight ship. There was Bruce Heezen, originally one of Doc’s protégés, who, along with Marie Tharp, had discovered the mid-ocean rift and had become a believer in an expanding earth. There had been a famous falling out between Bruce and Doc a few years earlier. There was Manik Talwani, Marc Langseth, Jim Heirtzler and Xavier LePichon, all accomplished marine geophysicists, who, in different combinations, had published several papers critical of the idea of seafloor spreading before the appearance of Walter’s magic profile. There were the seismologists, Jack Oliver and his protégés Bryan Isacks and Lynn Sykes, who had instantly jumped on the implications of Walter’s magic profile and published a series of classic papers outlining the fundamentals of the new global tectonics. And there was Neil Opdyke in the office next to Walter’s, a paleomagnetist and one time captain of the Columbia football team, who was an early believer in continental drift, perhaps the only one at Lamont before the appearance of Walter’s profile.

As I heard more stories about the events of the revolution the cast of characters quickly grew because the revolution had not started at Lamont. Harry Hess at Princeton was famous because he had written a seminal paper proposing the process of seafloor spreading in 1960. Perhaps most important from a magnetics point of view was the work of Fred Vine and Drum Matthews at Cambridge who originally proposed that marine magnetic anomalies at spreading centers contained a record of reversals of Earth’s magnetic field. They had hit the bull’s eye. One amazing aspect of their story is that they figured out the answer from data on the Carlsberg ridge, a slow-spreading ridge where the magnetic anomalies are much messier and harder to interpret than anomalies which form at fast-spreading centers such as the Pacific–Antarctic ridge, where Walter’s magic profile was eventually collected. I also heard that there was a paper by the Canadian scientist Lawrence Morley with virtually the same idea as Vine and Matthews, which had been repeatedly rejected by journals.
And there were the magnetic data sets from the Pacific Ocean collected in the 1950s by Ron Mason, Arthur Raff and Vic Vacquier at Scripps which in retrospect contained a beautiful record of seafloor spreading but whose origin was unknown at the time they were collected. These scientists had the misfortune of trying to interpret seafloor spreading anomalies before the phenomenon of reversals of Earth’s magnetic field was widely understood.

Most of the stories I had heard were incomplete. Yes, there was a famous falling out between Doc Ewing and Bruce Heezen but what exactly was it about? And yes, Talwani, Langseth, Heirtzler and LePichon at Lamont had initially rejected the Vine and Matthews interpretation of magnetic anomalies at mid-ocean ridges, but these were all very smart guys, so why did they reject it? And how was it that Fred Vine, a young, first year graduate student at Cambridge in 1963, was in a position to interpret the data set that Drum Matthews had just collected on the Carlsberg ridge? And how was it that he saw the answer? Several of Vine and Matthews’ colleagues at Cambridge were also slow to accept their interpretation. These are all fascinating questions because they are at the heart of understanding how plate tectonics was discovered, and more generally, how science unfolds. This volume explores the answers to these and many more fascinating questions.

The discovery of plate tectonics took place through many small steps and some enormous leaps of scientific insight. Frankel examines all of the steps, big and small, that were involved in the development of plate tectonics. Methodically, he first outlines where the science was at a certain point, and then describes the discovery that an individual or group made, and finally shows how the broader scientific world reacts to the new discovery. At each step he examines correspondence between the principals which casts light on the thought processes going on in the scientists’ heads. Frankel has directly communicated over the years with many of the principals to get them to explain the events in their own words. As he put the story together he would get back to the scientists to ask them to clarify the chain of events. He gives us short biographies of many of the individuals showing us, to the extent possible, how their backgrounds might have influenced their ability to make these discoveries.

There are many riveting points in this discourse. As a scientist, I am particularly fascinated by the Eureka moments, when the answer to a problem suddenly reveals itself to a researcher. There were many such moments in the plate tectonic revolution and they make for compelling reading. One of my favorites is when Hess, Wilson and Vine realize that there should be a symmetrical pattern of seafloor spreading anomalies on the Juan de Fuca ridge; Vine rushes to the library and brings back the publications of Mason and Raff and together they all absorb the implications of that remarkable magnetic anomaly pattern. A second inspiring Eureka moment is when Oliver, Isacks and Sykes are looking at the earthquake seismic data from the Tonga trench and realize that they are seeing the descending slab plunging through the mantle.

The plate tectonic revolution proceeded quickly once Pitman’s magic profile was published. LePichon reversed his stance seemingly overnight and wrote the first
comprehensive paper outlining Cenozoic global plate motions. Euler rotations, underpinning the theoretical basis of the motion of rigid plates, had implications for how the azimuths of fracture zones and the rate of seafloor spreading should vary along mid-ocean ridges. This was realized independently and roughly simultaneously by Dan McKenzie at Cambridge and Jason Morgan at Princeton. Morgan saw the light a few months before McKenzie did, but McKenzie was the first to publish. Frankel goes to great length to examine this interesting episode.

There are many aspects of the story behind plate tectonics of which I was unaware. For example, after reading Vine and Matthews’ paper on seafloor spreading, George Backus, then a young seismologist at Scripps, realized that a test of this concept was that spreading rates should get faster the further away you got from the pivot point between two plates until you were 90° away. He wrote this idea up in a paper in 1964 and wrote a proposal to NSF requesting funds to collect magnetic profiles across the mid-ocean ridge in the South Atlantic to test this idea. It was not funded and the profiles in databases like the one amassed at Lamont eventually served the same purpose, but in retrospect Backus was way ahead of his colleagues in his thinking.

As I read through the volume I was also struck by the fact that so many of my first encounters as an undergraduate with plate tectonics in the late 1960s – J. Tuzo Wilson’s demonstration of transform faults; Bullard’s demonstration of the fit of the continents around the Atlantic; the second edition of Arthur Holmes’s *Principles of Physical Geology*; and poster-sized charts of the global seismicity pattern – were shared by other young scientists of the era. Reading this volume kindled some fond memories from this period of my life when I was just embarking on a career in science. Frankel has done a marvelous job of capturing the wonders of a scientific revolution.

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Abbreviations

AGU American Geophysical Union
AMSOC American Miscellaneous Society
ANU Australian National University
APW Apparent polar wander
FIDS Falkland Islands Dependencies Survey
GAD Geocentric axial dipole
GSA Geological Society of America
GSC Geological Survey of Canada
IGPP Institute of Geophysics and Planetary Physics
IGY International Geophysical Year
IUGG International Union of Geodesy and Geophysics
JGR Journal of Geophysical Research
Lamont Lamont Geological Observatory
MIT Massachusetts Institute of Technology
NIO National Institute of Oceanography (UK)
NRC National Research Council (Canada)
NRM Natural remanent magnetization
NSF National Science Foundation (USA)
RAS Royal Astronomical Society (UK)
RS1 Research Strategy 1
RS2 Research Strategy 2
RS3 Research Strategy 3
Scripps Scripps Institution of Oceanography
UCSD University of California, San Diego
UCLA University of California, Los Angeles
UCRN University College of Rhodesia and Nyasaland
USCGS United States Coast and Geodetic Survey
USGS United States Geological Survey
V-M Vine–Matthews hypothesis
WHOI Woods Hole Oceanographic Institute
Introduction

Volume III described the growth of marine geophysics/geology during the 1950s and early 1960s and how it led to the development of four hypotheses to explain the origin of mid-ocean ridges: Hess’s and Dietz’s seafloor spreading, Menard’s hypothesis of seafloor thinning, Heezen’s and Carey’s rapid Earth expansion, and the Ewing brothers’ hypothesis that ocean ridges are produced by small-scale mantle convection. Of them, only the Ewings’ was fixist. Although land-based paleomagnetism had showed (Volume II) that the Ewing brothers’ fixism, and hence their hypothesis, was incorrect, most marine geophysicists and geologists failed to acknowledge paleomagnetism’s confirmation of mobilism, and did not discard the Ewings’ hypothesis because it did not incorporate continental drift. Moreover, during the early 1960s, none of these hypotheses warranted acceptance because the gap between what was known about ocean basins and what was implied by the hypotheses was too great to eliminate any of them.

Volume IV begins by detailing how this gap was closed by the confirmation of the Vine–Matthews hypothesis (hereafter, V-M) and the idea of ridge-ridge transform faults. Vine and Matthews proposed their hypothesis in 1963, and the idea of ridge-ridge transform faults was separately put forth by J. Tuzo Wilson and Alan Coode in 1965. Their confirmation in 1966 yielded two difficulty-free solutions. By 1967 the vast majority of marine geologists and geophysicists, and many but by no means all land-based workers, had accepted seafloor spreading and continental drift, thus ending for them the sixty year continental drift controversy. However, continued rejection of mobilism had become doubly unwarranted; unwarranted because of mobilism’s support from land-based paleomagnetism and from marine geophysics/geology.

Despite the resolution of the drift controversy, plate tectonics, the eponymous crowning achievement of the revolution, had yet to be discovered, confirmed, and conceptually explicated, and it with these that the final chapter of this last volume is concerned. Plate tectonics was independently proposed in 1967 by Jason Morgan, and Dan McKenzie and Robert Parker. It combined continental drift and seafloor spreading into a precise kinematic theory of Earth’s tectonics. Plate tectonics divides
Earth’s surface into a small number of rigid plates whose motions relative to each other are describable with mathematical precision. Initially applicable only to Earth’s present-day tectonics, plate tectonics was soon applied to the past.

There are two abiding themes. Plate tectonics is a kinematic, not a dynamic theory. Both Morgan, and McKenzie and Parker adopted a version of seafloor spreading that jettisoned any direct tie between mantle convection currents rising immediately beneath oceanic ridges and mantle convection currents sinking immediately beneath trenches. Plate tectonics became the reigning theory of the Earth sciences even though its cause remained an unknown mystery. The old-time drifters’ plea that continental drift should be accepted even though its mechanism remained a mystery had gone unheeded by the vast majority of Earth scientists during the classical stage of the controversy, and then right on even into the mid-1960s, just so long as the occurrence of drifting continents remained, in the minds of most, an open question. Once drift of continents was unquestionably shown to occur, the old mechanism difficulty shrivelled away, becoming a phantom difficulty, an unsolved theoretical problem. So, ironically, mobilism gained acceptance even though its cause remained substantially unknown, although thought to involve mantle convection. Second, in the overall scheme of things, the importance of paleomagnetism did not diminish, but increased. Vine and Matthews’ hypothesis is based on the paleomagnetic idea of reversals of the geomagnetic field, and that newly created seafloor acquires a thermoremanent magnetization in the direction of the geomagnetic field as it cools through its Curie point. Determining the past history of ocean basins and coordinating the formation of ocean basins relative to each other are based on matching and dating magnetic anomalies from different ocean basins, the anomalies being tied, as they were, to reversals of the geomagnetic field. In addition, paleolatitudes, be they of continents or ocean basins, cannot be determined without the use of marine magnetic anomalies and APW paths, and in the very best work APW paths and plate tectonics are used hand-in-glove to work out not only Earth’s tectonic history but also Earth’s climatic history, for which latitude is of overriding importance.

I should also like to underscore the important role computers began to play in the Earth sciences. This is not surprising. Vine, Morgan, McKenzie and Parker, just to mention four of the many whose work we shall examine, learned how to compute and write programs that became essential to the development and testing of V-M and plate tectonics.

As in earlier volumes, I shall describe how researchers acted in accordance with what I have identified as three standard research strategies (1, §1.13). These three research strategies are my retrospective description of how they went about their tasks, how they addressed their problems; the workers themselves did not recognize or say that they acted in this way. Research Strategy 1 (hereafter, RS1) was used by researchers to expand the problem-solving effectiveness of solutions and theories. Research Strategy 2 (hereafter, RS2) was used by them to diminish the effectiveness
of competing solutions and theories; RS2 was an attacking strategy used to raise difficulties against opposing solutions, and to place all possible obstacles in their way. Workers used Research Strategy 3 (hereafter, RS3) to compare the effectiveness of competing solutions and theories, and to emphasize those aspects of a solution or theory which gave it a decided advantage over its competitors.