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Historical Overview of Severe Convective Storms Research

by

CHARLES A. DOSWELL III
Cooperative Institute for Mesoscale Meteorological Studies
University of Oklahoma
Norman, Oklahoma

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ABSTRACT

An overview of the history of research related to severe convective storms is presented, with a particular emphasis on the connection between this research and forecasting. Forecasting and basic research in severe convective storms have been intertwined since the very beginnings of modern severe convective storms research – that is, since the end of World War II. There are good reasons for this interaction, clearly tied to the large societal impact such storms can have. Several major milestones in severe convective storms research and key field observation campaigns are reviewed and described in terms of the interaction between observations, tools, and models that was first suggested by Tor Bergeron. This interaction continues to the present, and continues to be a useful way to understand the development of progress in scientific understanding about severe convective storms.

1. Introduction

As the possibility of another field project related to tornadogenesis is being considered to follow up on the successful VORTEX campaigns (Rasmussen et al. 1994), I believe it is useful to reflect on the history of how we came to the present state of scientific understanding in the subfield of severe convective storms research. I see the history of this research as dominated by three main themes: forecasting-research interactions, special field observing campaigns, and an idea proposed by Bergeron (1959) that understanding moves forward most rapidly when observations, tools, and models all are being advanced simultaneously and interactively. Thus, I will review these themes briefly.

Corresponding author address: Dr. Charles A. Doswell III, University of Oklahoma / CIMMS, 120 David L. Boren Blvd., Suite 2100, Norman, OK 73072-7304.
Email: cdoswell@gcn.ou.edu

a. Forecasting-research interactions

Perhaps more than in many subfields within meteorology, the task of forecasting severe storms has been intertwined consistently with both basic and applied research. Operational forecasting errors can be subdivided into three broad categories: (1) those associated with either not using or improperly using existing scientific understanding of the phenomenon being forecast, (2) those associated with a lack of scientific understanding, (3) those associated with operational data sampling limitations.[1] Eliminating the first is an education and training matter. As challenging as that might be (Doswell et al. 1981), it is not within the scope of this overview. Dealing with the third involves budgetary limitations that also are not of interest here. Moreover, even assuming new observational capability can be implemented operationally, new data can reveal heretofore unobserved processes that require research in order to understand them to the point where forecasters can take advantage of the new data

[1] This assumes that the process is well-understood scientifically, but the data available in operations do not permit a proper diagnosis.

(Brooks et al. 1995). Finally, those forecasting shortfalls attributable to gaps in our understanding are associated with the need for scientific research. Forecasters have recognized the need for research from the very beginning of severe storm forecasting if they are to forecast such events successfully. Forecasting is also one effective way to find the limitations in existing scientific knowledge. Some readers might be surprised to learn that most of modern severe convective storms research has its roots directly in the operational forecasting community.

b. Special field observing campaigns

As noted in the preceding section, a major obstacle to achieving an adequate understanding of severe storms has been the relative coarseness of the standard, synoptic-scale observing systems. Severe convective storms are not observed adequately in an operational setting, which strongly inhibits any attempt to understand the processes that drive them, much less to forecast them. Therefore, at least in the modern (post World War II—hereafter, WWII) era, special observing campaigns designed to provide more observations than available operationally have been used for providing new insights into convective storms. These observational projects have been major mileposts along the historical research path for severe convection. On several occasions they coincided with the *first* applications of new observing systems to severe convective storms research that since have become standard observations in subsequent campaigns. Further, many new observing tools introduced in research field campaigns have been implemented eventually as operational systems.

c. Bergeron's triad

Finally, consider the concepts articulated by Tor Bergeron in his 1959 review of how progress was achieved in the subfield of synoptic-scale extratropical cyclones. Bergeron proposes that the most rapid progress in meteorological science is associated with simultaneous advances in *observations, tools, and models*.[2] As I will attempt to show in what follows, his assessment also applies within the context of severe convective storms. The meaning and importance of observations has been explained already —the introduction of new observing systems always has been an important component of advances in scientific understanding. By "models" Bergeron means not just numerical models, but also includes dynamical, statistical, or even conceptual models. And by "tools" Bergeron means such things as computers, new analytical and diagnostic methods, and any other mechanism for using observations and models in ways beneficial to understanding.

These three themes are evident herein as I present what I consider to be most important milestones, marking the beginning of important changes in our scientific understanding. Section 2 presents a brief summary of severe storms research prior to the so-called Thunderstorm Project, and section 3 is devoted to the Thunderstorm Project itself. In section 4, events leading to the rebirth of severe storms forecasting and research are reviewed, and section 5 is focused on the evolution of the National Severe Storms Project. As discussed in section 6, something of a revolution in severe storms research occurred in the 1970s, related to the development of new observations, tools, and models. Section 7 reviews the birth of the notion of mesoscale convective systems, while section 8 considers the significance of the Verification of the Origins of Rotation in Tornadoes Experiment (VORTEX) and its relevance to the future of severe storms-related research.

As a participant in the latter stages of this history, my personal perspective inevitably colors my perception of historical events. Any written history represents a selection of events by the authoring historian(s) and so necessarily will leave some things out that others will feel are of equal or greater significance compared to the ones actually chosen. Despite those possible shortcomings, I have tried to include most of what I hope the majority would agree are the primary events. If I have overlooked what someone else considers to be important, I can only apologize in advance.

2. Pre-Thunderstorm Project research

For most of the history of meteorology, from its beginnings in the distant past up to the dawn of the computer era, models were predominantly conceptual. Analytic mathematical solutions are not feasible for most meteorological situations, except when making severely restrictive assumptions. Thus, meteorology of the time before the end of WWII could be characterized as a collection of relatively sparse observations, a

[2] Alternative concepts for this exist—see Hoskins (1983) or Shapiro et al. (1999) for examples of different descriptions of how progress in meteorology is made.

small set of mathematical models describing highly idealized versions of real atmospheric processes, and a host of conceptual models with highly variable physical and observational support. It was at most a distant dream when Vilhelm Bjerknes (1904), Lewis F. Richardson (1922), and others first considered weather prediction as a process that ultimately could be based on a direct application of Newtonian physics (see also Charney et al. 1950; Monin 1969).

Severe convective storms research was hampered by the general intractability of the equations governing the physical processes and by the complete absence of an observational basis for developing advanced understanding. See Galway (1989) for a review of some of the early research work in the United States Weather Bureau (hereafter, USWB) on severe storms. The following selection of individuals is at best only a representative sample of important contributions to severe convective storm science before the Thunderstorm Project.

a. 18th century - B. Franklin

Although most famous for the kite flight used to provide evidence that lightning was associated with electricity, Benjamin Franklin was also a keen observer of the weather. He was among the first to observe that weather systems moved along generally from west to east, producing weather in one region before going on to produce comparable weather farther along their track (see: http://sln.fi.edu/tfi/exhibits/franklin.html). This simple and, from a modern viewpoint, apparently self-evident proposition laid the basis not only for forecasting, but also for the notion that one could seek to understand the processes responsible for producing the sensible weather, including convective storms.

b. 19th century

i. Espy-Redfield controversy

James Pollard Espy and William C. Redfield carried on an acrimonious argument in the 1830s about the nature of cyclones. Espy's concepts, developed through consideration of the effect of temperature on density and, therefore, pressure, were known as the "thermal theory of cyclones" (see Kutzbach 1979). Redfield was a proponent of the effects of centrifugal forces on cyclones, perhaps because his ideas were strongly rooted in his studies of tornadoes. Modern notions that dynamics are scale-dependent were unknown at the time, of course, and so these theories were thought to be equally pertinent for *all* cyclones, from dust devils to tornadoes, hurricanes, and synoptic-scale extratropical cyclones.

Those engaged in current scientific controversies about competing hypotheses might do well to reflect upon how science now views the Espy-Redfield debates. That is, the thermal and centrifugal aspects of cyclones were subsequently reconciled as being elements of a more complex understanding of cyclones than envisioned by either Espy or Redfield. In a real sense, they were both partially right and partially wrong, as we now see the conflict between them.

ii. J. P. Finley

John Park Finley was an officer in the Army's Signal Corps during the latter part of the 19th century, whose contributions to the study of tornadoes were documented in detail by Galway (1985a,b). Galway provides an extensive biography of Finley and references for his contributions. Finley collected tornado reports to begin development of a tornado climatology in the United States (hereafter, US) and even attempted to make tornado forecasts (see Murphy 1996 for a discussion of his pioneering forecast verification scheme). Unfortunately for the science, Finley's military superiors eventually chose not to support his research—his tornado studies were terminated abruptly on orders from his commander. From the Finley era until after WWII (see below), tornado forecasts virtually were forbidden by the Weather Bureau. As a consequence, tornado and severe convective storm research in the US was limited correspondingly, perhaps owing to the absence of any potential for application to operational forecasting.

iii. A. Wegener

Best known for his proposed idea of continental drift that was rejected utterly by his contemporaries, only to be vindicated with observations of sea-floor spreading in the 1960s, Alfred Wegener was also a German meteorologist interested in tornadoes in Europe. He collected tornado reports in Germany and wrote a book about them (Wegener 1917) that, among other things, noted that tornadoes over land (windhosen) were physically identical to those over water (wasserhosen) and gave a recognizably modern definition of a tornado:

> "… large vortices with vertical axis extending from the base of a cumulonimbus cloud to the surface, visible completely or in part through condensation or, in the lower part through dust, in the form of a pendant

cone, funnel, hose or column. In a track typically on the order of hundreds of meters wide, with intense convergence towards the region of strongly reduced air pressure around the vortex axis, they in general cause damage of a kind not observed in even the strongest larger scale storms." (Wegener 1917, p. 5).

As shown by Dotzek (2001, 2003), Wegener's tornado reporting is something of an anomaly in the record of tornadoes in Germany—that is, in Germany as in much of Europe (and even in the US at the time), tornado reporting was not done systematically. Rather, the reports collected by interested individual researchers constituted the majority of the record. There was no consistent, sustained effort to collect and maintain an archive of tornado events.

c. early 20th century

i. J. Letzmann

Johannes Letzmann was a tornado researcher in Estonia (see Peterson 1992a,b). Like Wegener, he collected European tornado reports and studied the events in considerable detail, including analysis of the observed damage to infer the near-surface airflow. He also studied physical models of vortices in the laboratory to try to understand the physical processes. See Peterson's biography for an extended set of references to Letzmann's considerable publications.

ii. C. W. B. Normand

Sir Charles W. B. Normand, who spent a good deal of his career in India, is mentioned here because of his clear and still valid articulation of the concepts of parcel instability and latent heat release (Normand 1938), key concepts in understanding deep moist convection, as well as in application to severe storm forecasting (see the review by Schultz et al. 2000). The roots of simple parcel theory of convection go back to earlier times, of course, with work by Rayleigh, Benard, and others. But the development of our modern understanding of how this applies to deep convection, involving as it does the release of latent heat from condensation, is mostly the result of early 20th century meteorologists, including Normand as well as Carl-Gustaf Rossby (1932) and others.

iii. Showalter and Fulks

Although relatively little research on severe convection was done in the US during the early part of the 20th Century, A.K. Showalter and Joseph R. Fulks (1943) were among a small number of USWB forecasters who had investigated tornadoes and the meteorological settings in which they occurred. This work by Showalter and Fulks was incorporated subsequently into post-WWII studies related to severe storms forecasting (as described by Galway 1992), but it also represents an early application of the analytical tool of using multiple case studies for finding out what are representative settings for severe convective storms. This methodology was motivated by an interest in forecasting but it also provides an alternative perspective to compare with highly-instrumented special field observing campaigns. By using routinely available but relatively coarse observations, common large-scale features could be determined that provide both potential forecast parameters and an understanding of the processes that set the stage for severe convection.

3. The Thunderstorm Project

Aircraft played a pivotal role in WWII, and it became obvious to the military that the existing scientific basis for forecasting the hazards to aircraft posed by deep convection was very limited, indeed. Therefore, it became a matter of national importance to have a better understanding of thunderstorms. The prospects for adding to the existing understanding were reasonably good because the war had spurred the development of new technology, notably radar and computers. For severe convective storms research following WWII, the application of computers was still mostly a dream[3], whereas radar already was seen as an exciting new tool of observation.

a. Radar and in situ aircraft observations

During the war, radar was developed and used successfully for detecting aircraft and ships, but at times was "cluttered" with weather echoes. Whereas military radar applications considered these echoes as an undesirable annoyance, a few meteorologists began to realize that radar offered a previously nonexistent capability for monitoring the development and evolution of deep convection. Radar became an important

[3] For synoptic-scale meteorology, however, the early developments in numerical weather prediction via computer models were already underway shortly after WWII (Thompson 1961; Shuman 1989; Persson 2005a,b,c)

observational tool in the Thunderstorm Project for this reason. Many aspects of the major concept to come out of the Thunderstorm Project—*the thunderstorm cell* (Fig. 1)—were based heavily on radar observations.

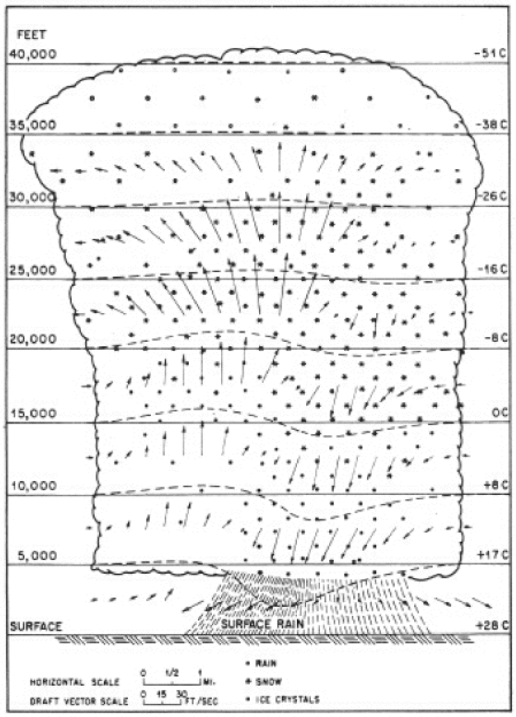

Figure 1. Schematic of a mature thunderstorm cell from Byers and Braham (1949)

Arguably, the most important element in developing the thunderstorm cell concept was the *combination* of the radar observations with in situ measurements by aircraft penetrating the storms at several levels in "vertical stacks." This permitted the identification of organized up- and downdrafts as observed by aircraft, relative to radar-observable precipitation-filled regions of the thunderstorms.

b. Aircraft safety

The timing and location of the two different field phases of the Thunderstorm Project—during the summertime in Ohio and Florida—were chosen, according to Braham (1996) "… on the basis of thunderstorm frequencies and the presence of military facilities capable of supporting the project." A major goal of the project was to determine the extent to which radar information could be used by aircraft to avoid the most dangerous parts of thunderstorms. Braham (1996) goes on to state, "No storm was to be avoided because it appeared too big." Because the field campaigns were done during the summer, most of the observations were of nonsevere thunderstorms. Therefore, aircraft participating in the penetrations were not so much at risk, although occasional damage from hail still occurred. The important role of in situ aircraft observations in developing the basic understanding of convective storms was established firmly in this project. Although the capability of research aircraft has expanded considerably since the Thunderstorm Project, this effort was the prototype for using research aircraft, in combination with radar, to provide detailed observations of severe convective storms.

c. Thunderstorm Project mesonetworks

The decision to provide a special network of surface and supplemental radiosonde observations during the Thunderstorm Project was also a critical one. These data provided evidence of gust fronts and their associated outflows, low pressure at the base of updrafts, and high pressure at the base of downdrafts. Such concepts were to be developed in much greater detail by subsequent projects (see sections 4a and 4c), but were not the primary focus of the Thunderstorm Project's findings. Nevertheless, special observing networks at the surface and extra radiosonde ascents became the "industry standard" for most such projects to come. We still consider augmented surface networks and supplemental soundings to be essential for field campaigns to this day and expect them to remain so well into the future.

4. The development of operational severe storms forecasting

a. M. Tepper and the USWB mesonetwork

Following the Thunderstorm Project, Morris Tepper, a USWB scientist, proposed that a mechanism he called a "pressure jump" (Tepper 1950) might an important factor in squall lines, then thought to be the primary mode for severe convective storms. Tepper speculated that the pressure jump, conceptually modeled as a hydraulic jump, might somehow create tornadoes as pressure jump lines intersected. He needed more data to validate his ideas, so with the support of Dr. Harry Wexler[4], a mesonetwork of surface observation sites including micro-barographs was established in 1951 on the Plains to help provide

[4] Wexler was then USWB Director of Meteorological Research; see: http://www.history.noaa.gov/giants/wexler.html

the needed data. Initially, this was called the "Tornado Project" (Galway 1992). Tepper's ideas about the pressure jump idea were discarded eventually after thorough analysis of the data, but the mesonetwork established to explore this concept ultimately was to have far-reaching impacts on severe convective storms research. Again, the primary impetus and funding support for this project was driven by the needs of operational forecasters. The USWB of the day had the foresight, will, and resources to carry out research projects in support of forecasting—to provide its scientists with the means to obtain the supplemental, non-operational data necessary for exploration of new ideas. In fact, the USWB even employed "research forecasters" as a part of forecast operations in some field offices, which could be seen as forerunners of today's Science and Operations Officer (SOO) in the National Weather Service (NWS). However, today's SOOs have diverse duties besides research and operational forecasting, whereas the USWB research forecasters were researchers first, and forecasters second.

b. E.J. Fawbush and R.C. Miller and the formation of SELS

Most severe storm forecasters and researchers should know the story of the first US tornado forecasts since J.P. Finley. An entire issue of *Weather and Forecasting* (August 1999) was devoted to papers contributed in celebration of the 50th anniversary of that famous forecast of 25 March 1948. Briefly summarizing, the Air Force mandated a tornado forecasting program after a tornado devastated aircraft stationed at Tinker Air Force Base, OK, several days before the famous first forecast. Ernest J. Fawbush and Robert C. Miller were Air Force officers tasked with the job of coming up with methods to forecast severe weather, especially tornadoes. Once the fortuitously successful first forecast was made, Fawbush and Miller went on to become pioneers in severe storm forecasting-related research. Since this was a topic that mostly had been abandoned in the US after the Finley episode many decades earlier, there was so little known about severe convective storms that everything Fawbush and Miller (and their colleagues) did amounted to *basic* research, whatever its motivations. Fawbush and Miller published papers on their work (e.g., Fawbush and Miller 1953; 1954a,b), and the effort eventually was summarized in Miller's forecasting guide (Miller 1967, revised and reissued in 1970).

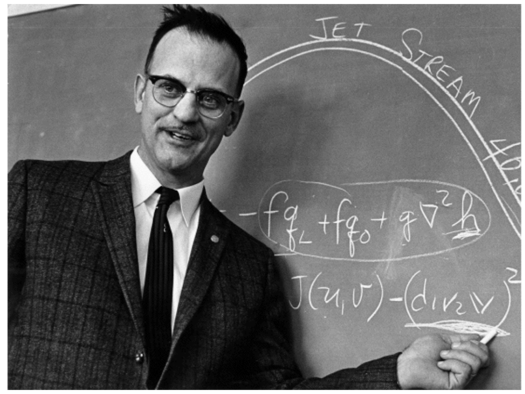

Figure 2. Don House at the chalkboard.

Figure 3. The TRAP research P-51 aircraft.

The development of Fawbush and Miller's forecast team, which would come to be known as the Air Force Military Weather Warning Center, soon forced the USWB to reconsider its policy of not issuing tornado forecasts and eventually to create its own Severe Weather Unit [or SWU, which soon was renamed the Severe Local Storms (SELS) unit] partway through the spring storm season of 1952. See Corfidi (1999) for details of the history of this unit. It was recognized right after the unit commenced forecast operations that research was needed, since so little was known about severe convective storms at the time.

Donald C. House (Fig. 2), the second head of the SELS unit, was a strong proponent of research in support of severe storms forecasting (which effectively amounted to *basic* research). Soon there were several research forecasters working with the regular forecast team. This established a tradition for forecast-related research in SELS that has continued to this day.

Among other things, research aircraft (Fig. 3) were acquired to gather data around tornadic storms, although few substantial results ever came from the Tornado Research Airplane Project (TRAP—see Lee and David 1961). McGuire (1962) reported on dryline studies based on data gathered with an instrumented P-38 aircraft. Most of the work with research aircraft at NSSP/NSSL came to be focused on storm-generated turbulence as an issue for aircraft operation (e.g., Fankhauser and Lee 1967). Research forecasters from SELS eventually formed the nucleus of the National Severe Storms Project (NSSP). Galway (1992) provides a discussion of this evolution.

c. T. Fujita's early U.S. research projects

The late Tetsuya Fujita[5] possessed an uncanny ability to ability to use limited information innovatively, allowing him to suggest physical processes that could explain the observations. Prof. Horace R. Byers (of Thunderstorm Project fame) invited Fujita to the University of Chicago, where he was to work for the rest of his professional career. Fujita's contributions over the course of five decades are so numerous, it would be impossible to give him full credit in this summary [see his autobiographical work (Fujita 1992) and Forbes and Bluestein (2001)]. My goal here is to illustrate his earliest work to consider how influential it was on what followed.

i. Mesonetwork analysis

When Fujita arrived in the US, he became interested in the data from the Midwest mesonetwork and was asked by the USWB to participate in data analysis. He began to apply the analytical tools he had created in Japan to these data, leading to new conceptual models of mesoscale convective system structure (e.g., Fujita 1955). His mesoanalysis techniques [first published in a USWB "research paper" (Fujita et al. 1956) and later summarized in Fujita (1963)—see Fig. 4] remain a cornerstone of such research to this day, as do his conceptual models of the structure and evolution of convective mesosystems. One by-product of this work was the realization that Tepper's pressure jump model was inappropriate for what was being observed (convective outflows), so Fujita's work helped to discredit the very application of the Midwest mesonetwork that originally had motivated its creation. This opened the door for many more exciting projects based on the data and the analytical tools Fujita had pioneered. As often happens in science, the development of new observations and the analysis tools to make use of them followed paths that were not originally anticipated, but led eventually to the creation of new process models that summarized new understanding.

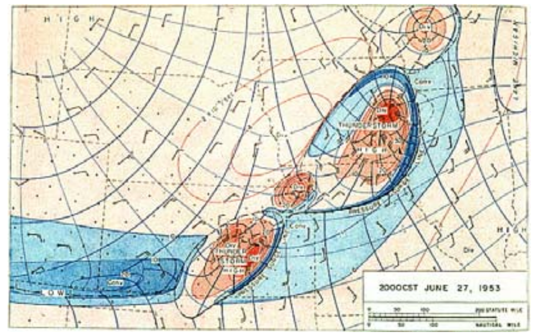

Figure 4. An example of an analysis by T. Fujita of Midwest Mesonetwork data, from Fujita et al (1956).

ii. Tornado photogrammetry

Another arena in which Fujita developed new techniques was the use of film images (both still and moving picture film) to produce quantitative analyses (Forbes and Bluestein 2001). He was not the only one doing so in the late 1950s (see e.g., Hoecker 1960—Walter H. Hoecker, Jr. was a USWB employee), but his studies of the Fargo tornado of 20 June 1957 were so comprehensive and innovative, they influenced a whole generation of severe storm meteorologists, myself included. Fujita used the multitude of storm images from that event, as well as conventional observations, to develop a conceptual model of the tornadic storm (Fujita 1960) that would come to be recognized as a *supercell* convective storm. Since the existing meteorological knowledge of such storms was so scanty at that time, Fujita had to develop new terminology to describe the observations; no terms then existed for what he was seeing. Fujita's terminology, including "wall" and "tail" cloud formations, survives to the present, although not without some controversy—Fankhauser et al, (1983a,b) proposed some different terminology, prompting comments by Doswell (1983) and Moller (1983). Existing definitions for the terminology of severe storm-associated cloud formations can be found in Glickman (2000).

d. USWB research forecasters

Robert G. Beebe was a USWB research forecaster who influenced many disparate aspects of severe convective storms research. He became involved with SELS early in its history, having transferred to SELS from a research

[5] His anglicized nickname "Ted" eventually was formalized when he became an American citizen and changed his name to Tetsuya Theodore Fujita in 1968.

forecaster position with the USWB in Atlanta, GA. Arguably his most well-known work, (Beebe and Bates 1955) was done in collaboration with Fred C. Bates (who later was affiliated with St. Louis University at the time of his untimely death in 1969). This study, aimed at understanding convective initiation, was clearly motivated by the needs of operational severe storm forecasters at the fledgling SELS unit. However, Beebe actively pursued a variety of studies involving other severe storms-related basic research topics (e.g., see Beebe 1958).

Dansy T. Williams was another SELS research forecaster who made many contributions to the NSSP, but who published relatively little in the formal literature—an exception is Williams (1954). His work, mostly summarized in informal reports (e.g., Williams 1963) was similar to that done by others who achieved more fame (e.g., Fujita), and was focused mostly on the analysis of surface observations of convective mesoscale systems using the mesonetwork observations. His apparent lack of interest in formal publication caused his relative obscurity in the annals of severe convective storms research, but his work was available to his peers at SELS and NSSP via informal publications, and so was influential.

5. The National Severe Storms Project

a. The schism

Even as research forecasters were brought in to SELS to carry out the tornado and severe storms related research, a rift developed between those primarily interested in research and those mostly involved in forecasting. At first, the forecasters and researchers were simply elements of a team striving to improve severe storm forecasting, but apparently the responsibility of all team members to do operational forecast shiftwork grew into a major dispute (J. G. Galway 1980, personal communication). By 1958, this rift resulted in the researchers and forecasters literally moving to different floors in the former Kansas City, MO, Federal Building on Walnut Street. Many of the forecasters apparently resented researchers for not working their share of forecast shifts, while most of the researchers felt they were wasting valuable research time when forecasting. Eventually, this dispute led to the departure of several researchers to other organizations (as described by Galway 1992) and the eventual move of the NSSP research group to Norman, OK, where they were soon to be transformed into the National Severe Storms Laboratory (NSSL).

b. K. Browning's work

At about the time the clash between SELS forecasters and researchers was deepening, Keith Browning participated in the 1963 NSSP spring field observing campaign involving the Weather Radar Laboratory (WRL) in Norman, OK. Under the supervision of Prof. Frank Ludlam,[6] Browning then was working with the Air Force Cambridge Research Laboratory, following up his Ph.D. dissertation studies of a severe storm that hit the town of Wokingham, England. Detailed observations of the Oklahoma storms of 26 May 1963 formed the basis for an important summary publication (Browning 1965) and several journal articles (e.g., Browning 1964) of considerable importance in establishing the concept of what came to be known as supercell convection. Browning's conceptual model was deduced mainly from radar evidence, inferring internal storm airflows from the structure and evolution of the radar-detectable precipitation echoes. This pioneering work, when combined with that of Fujita, who also had begun using radar information in innovative ways (e.g., Fujita 1963), provided a much enhanced understanding of the internal structure and evolution of severe storms, especially regarding the relationship between tornadoes and their "parent" thunderstorms. Browning also incorporated information about the interaction between such storms and their environment, notably emphasizing the relationship between supercells and vertical wind shear (see also Newton 1963), which we now recognize as an important insight.

c. NSSP's evolution into NSSL

i. Primary emphasis on radar development

The NSSP was characterized by internal turmoil and change from the mid- to late 1950s until the time of its conversion to the National Severe Storms Laboratory (NSSL) in 1964. Nevertheless, progress in basic understanding was being made, as evidenced by the very interesting paper[7] attributed to NSSP Staff Members (1963). The nominal anonymity of the individuals contributing to this paper may have been intended as a show of unity that actually did not exist.

[6] Prof. Ludlam, a keen observationalist, contributed an extensive review (Ludlam 1980) of atmospheric convection on many scales.

[7] The contributors' names are listed in a footnote.

A new research direction was assured when Edwin Kessler became NSSL's first director in 1964. Kessler felt that routine surface and upper-air observations were not likely to be very productive in understanding severe convection, as he believed that most of the useful information had been mined already from such data (E. Kessler 1974, personal communication). Therefore, NSSL was committed from its inception to special field campaigns with an emphasis on the development of radar as a primary observing platform. History surely suggests that this choice was a productive one, leading as it did to the development of Doppler radar—as a research tool of great importance and eventually as the major component of the operational warning system. However, the decision also solidified a disconnection between NSSL research and SELS forecasting. Not only were the severe storms researchers and severe storms forecasters now separated by hundreds of miles, but their objectives had become functionally disparate. Relatively little collaboration between NSSL researchers and SELS forecasters happened for two decades thereafter.

ii. Spring programs

Annual spring observing campaigns were the norm for NSSL in the years following its inception. Although the program was nominally national, its observational program was weighted heavily toward studying supercell tornadic storms in Oklahoma, with a strong emphasis on radar-based research and development. Thus, the NSSL research program came to be perceived as relatively narrowly focused, despite the existence of other, diverse projects within NSSL. Given that the observing system used for major NSSL research projects operated during a finite temporal window each spring and was fixed in space[8], the value of each year's observations was highly dependent on having storms occur fortuitously well within the network, at the time it was in operation. Only a few well-sampled storms were suitable to be chosen for major project studies. The observations mostly were recorded on analog strip charts and photographic film, along with stacks of teletype data and facsimile charts, so it was a major effort simply to process all the data into forms suitable for scientific analysis. Therefore, most of the spring program data accumulated in a warehouse and the vast majority of the data were only given cursory examination, if any. Ultimately, most of those data were discarded in the 1980s.

Of course, this does not imply that *nothing* useful emerged from the spring observational campaigns. What few comprehensive case studies that *were* done (e.g., see Charba and Sasaki 1971; Barnes 1974; Lemon 1976) formed the basis for much of the understanding of severe convective storms that we had coming into what I consider to be the "revolution" that began in the early 1970s. Nevertheless, most of that understanding was not based on large sample sizes.

6. The 1970s revolution

Three developments occurred in the 1970s that together constituted what I see as a revolution in the scientific understanding of severe convective storms. This revolution specifically illustrates Bergeron's notion that rapid advances in understanding occur when observations, tools, and models are advancing together. I will not attempt to detail precisely how these developments influenced each other—the interaction being sufficiently nonlinear that I consider it implausible even to suggest that I understand it in detail. Nevertheless, the period was one of rapid advance in our perceptions of severe convection.

a. Doppler radar and NEXRAD

As already indicated, a primary focus of NSSL research by 1970 was on the development of *Doppler radar* as a new observational tool. Although Browning's work demonstrated that a qualitative understanding of storms could be derived from considering the time and space distribution of radar-observable hydrometeors, the quantitative assessment of storm airflows required a new observing capability. Much information about the history of radar and its development in the context of severe convection can be found in Atlas (1990). The early case studies of tornadic storms as observed by the NSSL Doppler radar(s) demonstrated the value of Doppler radar in diagnosing severe storm structure and evolution (e.g., Brown et al. 1978). As a primary example, the Union City tornadic storm on 24 May 1973 became not only the subject of an NSSL report (see Brown 1976), but many of the constituent papers in that report

[8] The component observing systems of the annual spring observation campaign, especially the mesonetwork sites, might be moved from one year to the next.

were published in a special issue of *Monthly Weather Review* (in January 1978). Much of the thinking that went into subsequent studies of tornadic storms was heavily influenced by that single event. Not only was this among the most intensely studied severe convective storms in history up to that date, but it also was very influential in showcasing the potential of Doppler capability in operational severe storm and tornado warnings. Eventually, this led to the implementation of Doppler capability in the Next Generation Radar (NEXRAD) program of the NWS, which later became known as the WSR-88D (for Doppler) radar system that is used operationally today. Radar information available only in Oklahoma in 1973 is now routinely available across much of the nation, and in many places around the world.

Doppler velocity data offer valuable information, beyond that of reflectivity alone, about the structure and evolution of severe storms. In a research mode, using multiple Doppler research radars, the capability to process the radial velocity data to calculate the vector winds within storms was revolutionary—numerous papers were published providing detailed analysis of those windfields. A thorough listing of those works is impractical here—the articles and references in Atlas (1990) provide a review of how that quantitative capability revolutionized our perception of the internal processes within severe storms.

b. Numerical cloud models

The second component of the 1970s revolution is the development of sufficient computer power to implement *3-dimensional numerical cloud models*. Schlesinger (1975) was the first to present results from such a simulation, followed by Klemp and Wilhelmson (1978). The capability to produce numerical simulations with a notable similarity to real storms greatly magnified the ability of researchers to do quantitative assessment of the physical processes driving severe convective storms. For example, the perturbation pressure distribution in and around severe storms had been the topic of considerable interest (e.g., Newton and Newton 1959) and even controversy, but resolution of the conflicting ideas was not possible via in situ pressure observations. With numerical cloud models, it became plausible not just to diagnose with some accuracy the structure and evolution of the perturbation pressure in and around severe convective storms, but to have a quantitative physical understanding of the processes that governed the structure and evolution of that pressure field (Rotunno and Klemp 1982).

An important aspect of the use of numerical cloud models is that the early work was not an attempt to *forecast* deep, moist convection, but rather simply to *understand* the processes associated with it. Unlike numerical weather prediction on synoptic scales, numerical cloud modeling used unrealistic initial conditions (notably, horizontal homogeneity and unphysical initiating “bubbles”) to produce not forecasts but *simulations*. Using a vertical structure in the initial conditions drawn from “proximity soundings” (e.g., Beebe and Bates 1955), the relationship between the simulated storm and the environment in which it developed could be investigated. The classic early papers by Weisman and Klemp (1982, 1984) demonstrated ground-breaking capabilities with respect to showing how environmental conditions can influence the structure and evolution of convective storms. Among other things, this work provided a clear connection with the topic of severe weather *forecasting*, showing that basic research and operational applications of that research could develop together. See Wilhelmson and Wicker (2001) for a comprehensive review of how numerical cloud modeling has evolved.

c. Scientific storm chasing

The third component of the 1970s revolution is *scientific storm chasing*. Although storm chasing began earlier than this, it was not done in an organized way by atmospheric scientists, for the most part. The late Neil Ward gave an early account of a successful storm chase in 1961 (Ward 1961) and during the 1963 NSSL field observing campaign, several scientists engaged in a short storm chase (Donaldson and Lamkin 1964; Donaldson et al. 1965).

Another sort of scientific storm chasing began in 1966 when Charles and Nancy Knight began direct collection of falling hailstones in northeastern Colorado (C. A. Knight 2006, personal communication) for research (e.g., Knight and Knight 1979). This effort was extended to Oklahoma in 1973, in collaboration with the NSSL spring campaign and eventually to many locations around the world in conjunction with hailstorm studies.

Nevertheless, I consider scientific storm chasing to have begun in 1972 with the Tornado

Intercept Project (Golden and Morgan 1972), a joint effort by NSSL and the University of Oklahoma. The primary goal of this project initially was to film tornado debris clouds in order to determine tornadic windspeeds using photogrammetry. Retrospectively, I believe the main contribution of this initial storm chasing project was to demonstrate that by taking observing systems of any sort into the field (including, but not limited to, the human eye), a much larger sample of events could be achieved than with static instrument systems. It was revolutionary for meteorologists to be in a position to see real storms form and evolve, in order to compare their qualitative observations with the quantitative and conceptual models being developed, as well to observe the relationship between visual cloud observations and radar data. This latter contribution could be thought of as a form of "ground truth" for radar observations, which cannot collect data all the way to the Earth's surface. The discovery of the so-called Tornadic Vortex Signature during the Union City storm (cf. section 6a) exemplifies this capability, because the visual tornado was observed to be collocated with regions of strong gate-to-gate shear seen in the Doppler radar data (Brown 1976; Brown et al. 1978).

The use of mobile field observation teams collecting *quantitative* data as well as making qualitative observations is now recognized as a critical component of severe convective storm observation campaigns (including, of course, using research aircraft). By virtue of mobility, it has become feasible to add significantly to the sample size of severe storms on a consistent basis and, therefore, to overcome the tendency to overgeneralize from the limited samples resulting from fixed observational systems.

Finally, it is worth mentioning that post-storm surveys became a critical component of severe storms research in this period. This is a sort of mobile data collection, but of a very different sort than real-time observations while following storms in progress. Given that severe storms produce swaths of damage, it is important in understanding them to know just *what* severe events happened, *where* specifically they occurred, their *intensity*, and at *what time* they happened, in order to position those events in a storm-relative framework (e.g., Browning 1965).

In the 1970s, Fujita and his students performed many post-event surveys, including aircraft overflights, mostly for major tornado outbreaks. Similar surveys also were done in conjunction with comprehensive case studies on storms during the NSSL spring program (e.g., Davies-Jones et al. 1978). Post-storm surveys continue to be important, although the resources for conducting them recently have not been available in many cases (Speheger et al. 2002)

d. Studies tied to weather modification

The topic of weather modification associated with deep convection is controversial. I'm not going to divert attention by going down that path, but I need at least to mention two important field campaigns associated with exploring modification of deep convection in the 1970s that made significant contributions to our scientific understanding of storms.

i. The National Hail Research Experiment (NHRE)

Motivated by Soviet claims to being able to reduce the impact of hailstorms using seeding agents (e.g., as documented by Sulakvelidze et al. 1974 and Marwitz 1972d),[9] the National Center for Atmospheric Research (NCAR) carried out a multiyear field campaign to explore the possibility of modifying hailstorms beneficially via cloud seeding. This included aircraft penetrations to dispense seeding agents and to take in situ measurements. In the process, a large sample of severe storms was obtained, yielding important contributions to the scientific understanding of hailstorms as a consequence. A three part paper by Marwitz (1972a,b,c) had its roots in the Joint Hail Research Project, a project that immediately preceded NHRE's inception. Important work on hail microphysics (see Knight and Knight [2001] for a review) is an example of the many basic research findings relevant to severe storms that emerged from NHRE. Significant conceptual models of hailstorms emerged from this work (e.g., Foote and Frank 1983). Foote and Knight (1977) did a contemporaneous review of this project and another review can be found in Knight and Squires (1982a,b).

[9] See: http://www.ucar.edu/communications/staffnotes/9906/here.html

ii. The Alberta Hail Project

Also motivated by an interest in cloud modification, a separate multiyear field campaign was carried out in Alberta, Canada. This project also made important basic science contributions in addition to the focused work on hail production in severe convection. The classic work by Chisholm and Renick (1972) is but one example of the many contributions made during this project.

e. The Techniques Development Unit

Following the singular tornado outbreak of 03 April 1974, Allen D. Pearson (then Director of the National Severe Storms Forecast Center in Kansas City, MO) was able to obtain support for a small research unit attached to SELS, formed in 1976, called the Techniques Development Unit (TDU, Fig. 5), which was charged with the task of doing research in support of SELS forecasting operations. I was one of the charter members of TDU, following the completion of my graduate studies at the University of Oklahoma, during which I had been employed at NSSL. The first Chief of TDU (Joseph T. Schaefer) and Les Lemon also had been NSSL scientists before joining the unit. Thus, the TDU was a first step toward re-establishing the severed connection between the research and forecasting components of severe convection studies. In contrast to the research forecasters of the 1950s era, TDU scientists were willing from the very start to work forecast shifts. This was viewed as a way to stimulate research that actually would be useful in forecasting, by acquainting researchers with the real-world challenges of diagnosing and predicting severe storms. For the most part, instead of a rift between them, a collegial relationship developed between the researchers and the full-time forecasters, based on mutual respect.

In the early 1980s, the TDU mission shifted away from applied severe storm research, moving instead into management of, and software development for, the new workstation technology that was being adopted for operational use. This resulted in personnel changes for the TDU, creating another hiatus in a full research-operations interaction that was resolved by the relocation of the operational forecasters with the researchers at NSSL (see the end of section 7, below).

Figure 5. The original staff of the Techniques Development Unit.

f. Windstorm research

The occurrence of nontornadic "straight line" winds in convective storms always has been recognized as being of great importance and therefore the subject of considerable research. During the 1970s, it became clear that many severe windstorms have a quite different character from the supercells that produce a large fraction of tornadoes and very large (say, > 5 cm diameter) hail events.[10] As discussed by Wakimoto (2001), the processes that produce strong downdrafts are quite distinct from those that produce strong updrafts. During the 1970s, as the result of a series of fatal weather-related aircraft crashes, Fujita (1978) began to investigate these events. In the process, he drew attention to a special type of downdraft for which he coined the term "downburst" and also noted the "bow echo" configuration seen on radar in some windstorms. He pointed out that not all storms producing important small-scale downbursts (or "microbursts" as Fujita described them) are associated either with strong updrafts or much precipitation and might not produce any lightning (Fujita 1983). It was something of a conceptual breakthrough to realize that storms capable of producing significant surface outflow winds can appear relatively innocuous on radar. Thus, Fujita proposed two archetypes for potentially damaging windstorms—the wet and dry microburst storms. This has led to much subsequent research and the awareness of microbursts has reduced substantially their impact on commercial aviation.

[10] Supercells also are capable of producing severe winds, however, as well as hail and tornadoes.

7. The 1980s – Mesoscale convective systems

By the late 1970s, the use of geostationary satellite observations had become more or less routine, and those observations provided a valuable new perspective that could be combined with the convective system studies done by Fujita and others using mesonetwork data. By observing the appearance of convective systems over their life cycles, a small proportion of convective systems were seen to be larger and much more organized than most (Fig. 6). These were recognized by Maddox (1980) to be very important in terms of the weather they produced—notably, the severe weather produced in their formative stages, their tendency for persistence well after sunset over the US plains, and their significant contributions to warm season precipitation in the central US (Fritsch et al. 1986). The largest, most circular, and most persistent of these came to be called mesoscale convective complexes (MCCs) by Maddox (1980), but this soon was generalized to other morphologies under the similar name mesoscale convective systems (MCSs—Zipser 1982). Maddox (1983) found that MCCs often occurred in zones of strong warm air advection at low levels, in the exit region of a low-level jet stream that had been augmented by the nocturnal boundary layer wind maximum. This had been recognized, at least in part, much earlier by Means (1944, 1952), but the new satellite observations made this relationship much more evident. This work has been of fundamental importance in linking what had been learned about the internal structure of MCSs with synoptic-scale processes.

During this time, it was observed that many MCSs were associated with squall line structures on radar, some of which showed a bow echo structure. In a few cases each year within the US, MCSs produce an extensive swath of severe weather, mostly high winds. A well-known example is that of the 04 July 1977 storm in Wisconsin documented by Fujita (1978, p. 48ff.). These have come to be called "derechos" (Johns and Hirt 1987). The societal impact (at least in terms of property damage) of such events can exceed even violent tornadoes (Ashley and Mote 2005) and this alone makes MCS-associated windstorms an important research topic.

The MCS concept has become firmly established in understanding deep convection in the tropics, as well, even though most tropical

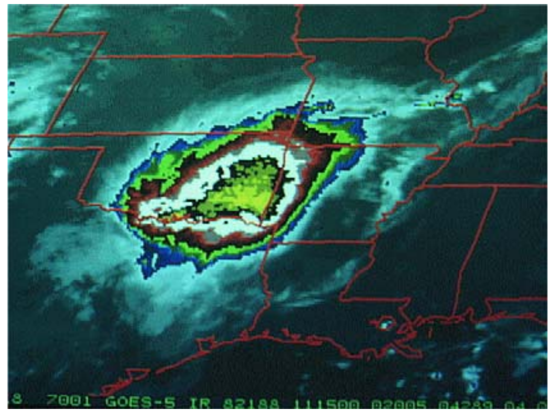

Figure 6. An enhanced infrared satellite image of a mesoscale convective complex.

convection is not severe by official standards (Barnes 2001). Tropical and midlatitude convective systems are similar but not exactly the same, owing to the relatively important contribution by the Coriolis force in midlatitudes, which is not typically important for tropical convection (ignoring tropical cyclones, of course). This connection between tropical and midlatitude MCSs has had important implications for the use of mobile observations (see section 7b).

a. APCL research

The seminal work on MCCs and MCSs began in what was then known as the Atmospheric Physics and Chemistry Laboratory (APCL) in Boulder, CO, headed by Charles F. Chappell (formerly a SELS forecaster). This research group also included Lee Ray Hoxit, Robert A. Maddox, J. Michael Fritsch, and Fernando Caracena, among others. The APCL scientists were stimulated by the 1976 flash flood in nearby Big Thompson Canyon to consider the meteorological aspects of flash flood events (Maddox et al. 1978; Caracena et al. 1979). Although flash floods officially are not considered "severe" weather in the United States, nevertheless they are convective weather events with considerable societal impact. APCL research showed that many flash floods (but by no means all) were associated with MCSs. It also was found they occurred in synoptic settings often perceived by forecasters before the event as relatively benign with respect to severe weather.

This has not only transformed our understanding of flash floods but has created the opportunity to apply that knowledge to forecasting them.

b. Field projects using aircraft

Tropical convection often occurs over the oceans, far from any land-based network of observations. Hence, efforts to learn more about such convection (including, but not limited to tropical cyclones) have been dominated by the use of instrumented aircraft—mobile observing platforms. Aircraft commonly thought of as "hurricane hunters," such as the P3 Orion and the C130 Hercules, have been used extensively for obtaining observations of tropical convection in such projects as the GARP Atlantic Tropical Experiment (GATE) in the tropics (Barnes 2001). In the 1980s, some of the same aircraft and the scientists accustomed to using them to study tropical convection were involved in field projects aimed at midlatitude MCSs over the plains. For example, the so-called PRE-STORM project of 1985 (e.g., Zhang et al. 1989) involved extensive use of research aircraft (that long had been used to probe tropical cyclones) as part of a coordinated effort that also included other observing systems. This work has revolutionized our understanding of the basic physical processes in MCSs, leading to, for example, the recognition of the so-called rear inflow jet (Smull and Houze 1987; Weisman 2001). Moreover, as noted above, aircraft have become an important component of many field observation campaigns in midlatitudes. Planes with radars, including Doppler radars, have added considerably to our capability to diagnose flow fields in MCSs, as well as to provide in situ observations (e.g., Wakimoto et al. 2004). Microphysical observing systems on board aircraft penetrating severe storms add an important dimension to our understanding, heretofore mostly unavailable and yet potentially critical in the processes associated with severe storms (Kessler 1969). Much remains to understand about the connection between microphysics and storm dynamics (e.g., van den Heever and Cotton 2004).

c. Big changes at NSSL

With the retirement of Edwin Kessler as NSSL Director in 1985, Robert A. Maddox became NSSL's new Director and soon instituted a change in direction for NSSL research. Rather than being "Oklacentric" (a derogatory term sometimes applied informally to severe storm studies based in Oklahoma), Maddox expanded the definition of "severe storms" to include hazardous weather in almost any form, including winter storms, heavy rainfall leading to flash floods, and so on. This also meant that the range of research studies was expanded to include the entire nation throughout the year, not just the southern Plains in the spring. Field campaigns were no longer rooted in the vicinity of central Oklahoma, carried out automatically in the same general location every year. Instead, field observation campaigns were to be focused on scientific questions that might take NSSL scientists outside of Oklahoma at different times of the year, often involving aircraft and other mobile observing systems. This wider vision of what constitutes a severe storm at NSSL continues to the present. Given that Maddox had roots in forecasting, having been affiliated early in his career with the Air Force's Military Weather Warning Center and Robert C. Miller (of Fawbush and Miller fame), the stage was set for an eventual reunion between NSSL and SELS. This was realized in 1997 (Corfidi 1999), following the renaming of SELS as the Storm Prediction Center (SPC) in 1995, such that the SPC operation eventually came to be collocated within the NSSL building.[11]

8. VORTEX and beyond

The Verification of the Origins of Rotation in Tornadoes EXperiment (VORTEX) took place in the springs of 1994 and 1995 (Rasmussen et al. 1994). This program represented both a return to focused field operations in Oklahoma (broadened to include nearby states, however) for the purpose of understanding tornadoes, and a new beginning for such field campaigns. Rather than being minor players in the field, the mobile observing capabilities:

- in situ and radar observations from research aircraft,
- a "mobile mesonetwork" (Straka et al. 1996) of sensor-equipped vehicles to bring surface observation capability to the storm, rather than waiting for the storm to occur within a fixed network, and
- mostly in 1995, mobile radar capability in the form of the Doppler on Wheels (DoW—Wurman et al. 1997),

were *essential* components. Even with this capability, however, the optimal situation would be when storms were within reach of the fixed radar systems in central Oklahoma. As it turned out, few such cooperative storms were sampled during the rather uneventful storm season of

[11] See http://www.spc.noaa.gov/history/early.html

1994. Thus, although relatively many storms were sampled, only a few were probed as comprehensively as hoped. Arguably the most comprehensive data collection for any storm during VORTEX was that for the 02 June 1995 storm near Dimmit, TX in the Texas Panhandle (Rasmussen et al. 2000).

A relatively unique aspect of VORTEX program design was the application of the principle of *testable hypotheses*. Rather than simply assembling a collection of observing systems and then hoping this would yield useful data, the scientific investigators were required to design their experiments to give their candidate hypotheses as rigorous a test as possible. It was mandatory that the project scientists provide a detailed plan for how the data collection should be done to maximize the value of the data for validating a particular idea. The candidate hypotheses, in turn, came from existing models of the processes associated with tornadogenesis—numerical models and conceptual models were both used in development of the experimental design. If some hypothesis, however interesting, could not be tested rigorously using available observing systems, it would not be included in the study.

My interpretation of the experiment's outcome is that we learned we collectively were not asking the right questions going into the field phase of the project. Experimental findings are always subject to different interpretations, but mine is that VORTEX succeeded brilliantly in showing us that we didn't understand tornadogenesis nearly as well as we thought. New ideas were needed, and some of them *could* be tested using the VORTEX datasets (e.g., Markowski et al. 2002). However, after the field phase of VORTEX, many VORTEX participants recognized that tornadogenesis is going to be a challenge for some time to come—hence, there have been annual small-scale follow-on programs using mostly the mobile radars and some of the mobile mesonet vehicles.

Now we are considering a reprise performance—VORTEX II—with new ideas (models) to test, new observing systems to use, and new analytical methods (tools) to apply. This is a sign to me that severe storms research continues to advance, as all the important components needed for rapid progress are moving forward more or less together.

a. Observations, tools, and models

The continuing technology improvement associated with observations is making mobile observing systems so powerful that future observing campaigns in a wholly-fixed network are unlikely. Our history has shown a spatially-fixed network results in small samples that inherently encourage us to overgeneralize from those results. Learning a great deal about a single storm can be very exciting, but if we don't know the similarities and differences between that event and others, we always will have important gaps in our overall understanding. Every weather event is unique, although it also has many aspects in common with other, similar events. Without *both* a detailed understanding of storms available on in a research mode, and a large sample size, it is going to be difficult to synthesize comprehensive new understanding. The mobility of research observation campaigns is going to allow both detailed knowledge from individual cases and the ability to generalize from a relatively large sample of events. I view this as a sine qua non for any proposed severe convective storms research in the future.

An interesting development has been the proliferation of automated fixed surface observation systems—in effect, *operational* systems with the resolution of the old "mesonetworks". The premier example has been and continues to be the Oklahoma Mesonet (Brock et al. 1995), but many special-purpose networks of automated surface observations have been developed for both private and public groups around the US, owing to weather sensitivities and the associated need for detailed weather data.

As remote sensing technology (e.g., satellite and radar) improves, ever-increasing detail will be available from it. Associated with this expansion in routine observing capability is the challenge of learning how to *obtain, compile, and use* all of the new information most effectively. Routine data collection will in no way replace the need for special observing campaigns, but as the resolving power of operational observing systems increases, it will become possible to have a truly comprehensive multiscale view of the atmospheric processes that govern severe convective storms.

All of this will require the development of new tools to analyze our data for enhanced scientific understanding. There is every reason to believe that this will occur as a natural outgrowth of having new forms of data, but learning how to use those data is not automatic. Investment in new data streams does not magically produce new insights or forecast improvements. There is a learning curve associated with every advance in observational capability—that process is called *research*, and it must be supported with appropriate resources if the benefits of new observations are to be realized as quickly as possible. The way I believe Bergeron saw it, new observations stimulate ideas that make use of those data in original ways to generate improved models. Testing those models, and revising them as needed, produces analytical tools as well as revised models. New observations are needed to test the revised models and so, hopefully, this process continues indefinitely.

b. Bridging the gap?

In the time since the split between the SELS forecasters and the NSSP researchers, we have come full circle. SELS has become the SPC and is now collocated with NSSL. This will continue for the immediate future as the so-called National Weather Center (hereafter, NWC)[12] becomes a reality in Norman, OK. The NWC will include not only most of the federal research and operational forecasting staff but many of the related units affiliated with the University of Oklahoma. Will this proximity produce the synergies some have predicted?

If my reading of the history of research in severe convective storms tells me anything, organizational structure and proximity do not necessarily result in productive collaboration. *People* choose to collaborate, or not. If two or more individuals sincerely wish for a collaboration to develop, then it likely will develop despite whatever bureaucratic or physical barriers might be present. Those barriers can *hinder* that process, as in the time between the formation of NSSL in 1964 and the move of the SPC to collocate with NSSL in 1996, but they can't stop it entirely. Conversely, if people don't want to collaborate, no amount of forced proximity or top-down mandates can create productive interaction. It remains to be seen just how this coming fusion of research and operational groups within the NWC will influence the course of severe convective storms research for the future. Bridging the gap is where individuals can make a real difference. Leadership and facilities that encourage the staffs to pursue collaboration are important, but not essential.

In my experience, ideas for research projects simply pour out of everyday forecasting operations, for the simple reason that forecasting forces one to confront both what is known and is *not* known on a daily basis. The challenge is that operational forecasters typically do not have the resources to do cutting-edge research. A collaboration between operational forecasting and research always has seemed an obvious good thing to me; so obvious, in fact, I long have felt that any rift between them is not in the best interests of either the forecasters or the researchers. History tells me, unfortunately, that mostly lip service is paid to the ideal of a consistent collaboration between operations and research, despite the empirical evidence that when cooperation between them *does* arise on occasion, our field is advanced most effectively. The extent to which this collaboration develops always has been, and will continue to be, up to the individual researchers and forecasters. Individuals have changed the course of severe storms research in the past, both positively and negatively, by choosing either to work together, or to discourage collaboration, respectively. It is my belief that individuals will continue to have the power to make a difference in the course of research in the future, as well. Whether that difference will be positive or negative is up to the individual.

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[12] See http://www.nwc.ou.edu/

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REVIEWER COMMENTS

[Authors' responses in *blue italics*.]

Reviewer A: Robert H. Johns

Initial Review:

Recommendation: Accept with minor revision

Comments:

This is a very interesting paper dealing with historical severe storms research and the interactions between forecasters and researchers. The one thing that seems to be missing is that there is no information at all about the research that was done associated with damaging straight-line winds produced by deep convection. Since this is an important aspect in forecasting severe local storms, it appears that this type of research should be at least briefly mentioned in some of current paragraphs and one or more additional paragraphs should be added. This would involve the reasoning for the early microburst and downburst studies as well as Fujita's discovery of bow echoes. And possibly some later observational and model research could be mentioned.

I agree that this is something of an important oversight and have added some discussion of this topic. This is not a forecasting paper, however, although forecasting clearly has had a big role in driving the research for reasons discussed in the paper.

[Minor comments omitted]

It seems that there is another category that is not mentioned. In some cases the scientific understanding is known and has been learned from detailed field studies with temporarily added observations, etc (as mentioned in part b of the Introduction section). However, because of the lack of sufficient operational observations, it may not be possible to view the ingredients on a small scale that are associated with the newly understood scientific processes. In such cases, forecasting errors can also occur even though the processes are well known.

I agree, although bringing this up introduces some complications, because new data often lead to discovery of previously unobserved phenomena, which means new research is needed. I've included this because it is indeed a logical possibility. Actually, of course, if the process is understood scientifically, you can still make use of the conceptual models built up from non-operational data in subjective forecasting. This could lead to a whole discussion of how formally inadequate data can be used by a human forecaster, but I'm trying to avoid this, as the paper is not about forecasting, per se, but only about research.

Reviewer B: Leslie R. Lemon

Initial Review:

Recommendation: Accept with minor revision

Who better to write such a paper? I found the paper to be well written and enjoyable to read, especially because I have lived a significant portion of that history. It needs only minor editing with no further review prior to publication. The paper is an excellent historical overview, well organized and well presented. The length is about right but could even be expanded somewhat if need be.

[Minor comments omitted]

Comments:

Even though much of my review deals primarily with word changes here and there my personal perspective inevitably colors my view as his personal perspective colors his perception of historical events.

In the sections considering in situ aircraft observations and aircraft safety I am reminded that for a number of years during spring data collection programs at NSSL they used an aircraft control radar, an air traffic controller, and a flight director during their Project Rough Rider. These penetrations were used to estimate updraft strength, turbulence, as well as to gather precipitation microphysical information. Their rather aggressive use of aircraft led to some notable aircraft-storm encounters. In the early years (?) of the NSSP an F-100 penetrated a severe storm as vectored by radar and the flight controller and encountered giant hail that shattered the cockpit wind-screen and severely damaged the leading edge of the wings and tail section and injured the pilot. The aircraft barely made it back to Tinker AFB safely. This encounter led to changes in storms that were chosen for penetration. After that, aircraft avoided all regions of >50 dBZ. For several years thereafter a section of the leading edge of the wing of that F-100 was prominently displayed in one of the offices of NSSP/NSSL. [See, for example, the NSSL Tech, Memo # 36, "A preliminary evaluation of the F-100 Rough Rider Turbulence Measurement System".] But during June 8th, 1974, an aircraft was used to penetrate a Bounded Weak Echo Region and mesocyclone. Needless to say this was a hair-raising flight.

I have expanded some on the role of aircraft-based research. I hope this will be satisfactory.

In the last paragraph of section 4 where Dansy T. Williams is discussed and his informal reports are mentioned it might be of interest to the reader to mention some of those informal reports that appeared in the NSSL Tech. Memo. series. For example #s 3, 11, 17 (Analysis methods for small-scale surface network data.), 18 (The thunderstorm wake of May 4, 1961), and # 20 were authored or co-authored by him. Doswell then mentions Fujita and his informal reports. Interestingly enough, an NSSL Tech. Memo., (#16, Analysis of selected aircraft data from NSSP Operations) authored by Fujita came during those of Williams. Moreover, if one reviews the author list of the NSSL Tech. Memos. of that time period they read like the Who's Who of severe convective storms research.

Good suggestion -- it has been incorporated.

Note that under the discussion of Browning's work it should be noted that he not only inferred internal storm airflows from radar echo but he also explained the radar structure of storms in terms of the storm's environment and interaction of the storm with its environment.

In the concluding paragraph of section 5 Doswell mentions the few useful comprehensive papers to come out of the NSSL mesonetwork. There are two other JAS papers, Lemon (1976a,b) that were heavily drawn on for the L&D '79 supercell model. These papers and that of Barnes used the space-time extrapolation of surface mesonetwork observations and numerical objective analyses.

Added Lemon reference.

Additionally where Chuck discusses the impact of Doppler radar studies leading to the operational use of Doppler, I believe that the JDOP field program and report should be mentioned. Also perhaps the 1977 BAMS paper (Doppler radar application to severe thunderstorm study and potential real-time warning) by Lemon, Donaldson, Burgess, and Brown that summarized research findings using Doppler data and recommended Doppler radar use as a warning tool, be referenced. Earlier under section 3 the new technology of computers was mentioned as extremely important to the advances in research. Here, advances in computing and display technology proved critical as well. We were unable to compute phase shifts or display data in real time prior to the development of the "Octant Change Counter" at NSSL by Dale Sirmans (Sirmans et al, 1974). This then led to the development of the FFT processors and the Pulse-Pair Processor. Of course, computer development was also critical there. Finally, the WSR-88D was the first real "computerized" radar system that also used artificial intelligence and pattern recognition algorithms.

I can understand that Les wants certain things mentioned. I have deliberately avoided as much emphasis on radar aspects of research as Les would probably want, because much of this is summarized elsewhere, in my references. Hence, I'm disinclined to follow this suggestion.

Under Scientific Storm Chasing, the efforts made during the famous 1963 tornadic storm outbreak in central Oklahoma might be mentioned. The photos and observations made by Hardee and Donaldson and others of the "bell-shaped cumulonimbus" during those storms and used by Browning in his publications might also be mentioned.

Done.

Note that under that same section, the discovery of the TVS is mentioned. First the signature is not called the "Tornado Vortex Signature" but rather the "Tornadic Vortex Signature." We chose that name because the signature was noted aloft before the tornado developed in association with the signature and was sometimes detected aloft when no surface tornado resulted. The confusion over the signature name has been difficult to correct. Brown (1976) is referenced for the TVS when I believe the MWR paper by Brown et al., (1978): "Tornado detection by pulsed Doppler radar" is a better reference. Finally, the importance of mobile field observation teams is mentioned in this same area of the manuscript. Here I was reminded of the importance of storm damage surveys and related research. Perhaps they might be mentioned here as well.

Done.

Under Field Projects Using Aircraft, I believe an excellent reference would be that of Wakimoto et al, (2004) using the P3 and airborne Doppler radar to study the 2003 Nebraska supercell. It was that study that revealed a record 20 km diameter mesocyclone with the most intense rotational velocities yet recorded.

Done.

Finally, as I read this and the discussion of the NWC I was reminded of the need for some researchers to broaden their view, take off the blinders, and collaborate with others having differing specialties. I believe that some of us, such as myself, could afford to do just this. And for me, the NWC has already begun to do that.

Reviewer C: David J. Stensrud

Initial Review:

Recommendation: Accept with minor revision

This manuscript presents a history of severe convective storms research from the perspective of someone who has been actively involved in this research area for most if not all of his professional career. The overview is well organized and discusses a number of important issues. I particularly liked the recurring thread of how understanding advances best when observations, models, and tools are being advanced simultaneously. The forecast-research interaction theme also is well done and is nice to see highlighted. Thus, I have only a handful of comments to offer in the hope that the manuscript can be improved even further.

Comments:

1) Section 5c, part iii. In this section you discuss the annual spring field campaigns that were a part of the early NSSL experience. The general impression one obtains is that these efforts were not very useful and that only a few comprehensive case studies were completed. However, I think a lot more credit is due. During the 1960s NSSL scientists were developing and testing the early Doppler radar systems and learning what these tools could do for meteorology. For example, it was found that the 3-cm Doppler radar was not powerful enough to be used as part of a surveillance network and so a surplus 10-cm Doppler radar thus was acquired in 1969 (but was not operational until 1971). Efforts to produce objective analysis techniques were developed and tested, and surface mesonetworks constructed. Thus, during these early years a lot of effort was expended in developing and learning how to use the tools that were the foundation of the "1970s revolution." You mention earlier that understanding moves forward most rapidly when observations, tools, and models are all being advanced. I think during the 1960s the tools were being developed that were critical to the revolution of the 1970s. Thus, this decade may be sparse for severe storms research, but it was crucial to providing the right conditions for the success of the 1970s.

I don't disagree with this point, and have modified the text somewhat to mention the importance of the development of Doppler radar. However, I prefer not to go into much detail in this regard, as others have written about these developments at some length.

2) One important interaction that you have not discussed is the collaboration between the Oklahoma City National Weather Service Forecast Office and NSSL. The NWSFO moved to Norman during the late 1980s (?) to be next door to NSSL, and your initial office in Norman was in the FO. I seem to remember

NSSL displaying Doppler radar data in the FO in real time for support of warning operations prior to the availability of WSR-88D data. While I was not involved in many collaborative activities at this point in time, this is an earlier example of interactions between researchers and forecasters that provided benefits to the severe storms community prior to the collocation of the SPC with NSSL.

Given the structure of the existing manuscript, I find it somewhat difficult to insert this in some appropriate place. Although I obviously believe in the importance of this, I'm not sure it represents a major milestone in the research process. Hence, I've not followed the suggestion, with some lingering misgivings about not doing so.

3) Section 6c. You mention that scientific storm chasing began with the Tornado Intercept Project that had the goal of filming tornado debris clouds. You then mention the use of mobile field observations to provide quantitative data in the last paragraph. When did scientists start bringing data collection platforms beyond cameras into the field?

An interesting point. This is sort of hard to know … just what sort of system constitutes quantitative sampling during storm chases? I have discovered that the Knights began collecting hailstones as they were falling, for the purpose of studying them later in their lab, during 1966. There might be others, but I might not know of them.

[Minor comments omitted]