A Biased and Personal Description of GR at Syracuse University, 1951–1961

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Summary. In mid-century, General Relativity was largely in the doldrums. Though at the time I was completely unaware of it, there were perhaps only four or five active groups around the world working in GR; Hamburg (Jordan), London (Bondi), Princeton (Wheeler), Warsaw (Infeld) and Syracuse (Bergmann). I had the privilege and pleasure of being a member of the Syracuse group working under Peter G. Bergmann. I would like to describe some of the things that took place there, who were the active participants, who we interacted with, what was accomplished and finally conjecture what role we played in the revitalization of relativity in the late 1950s and early 1960s.

20.1 Preliminaries

As a preliminary remark I want to say that I am not an historian of science — I am (I think) a working mathematical physicist — and I do not know the modus operandi of historians. I once read a history book — the biography of Erwin Schrodinger — and, from it, I thought that historians were only interested in the well-known licentious behavior of physicists. But our good friend John Norton quickly and definitely informed me that I had been misinformed. So I came to this meeting with a talk prepared about general relativity (GR) at Syracuse University in the years 1951–61; completely leaving out the rich details that Norton thought inappropriate. But then listening to all the talks in the first three conference days, I had the realization that I still did not understand how historians of science understood history. Every talk in the first three days dealt with Relativity before 1950. So on the night before my talk, thinking that history ended in 1950, I redid my notes so that I could describe to you GR at Syracuse University before 1950. That did present a problem, since there was not much GR done at Syracuse University before 1950 — but I managed to make up some interesting facts. Then to my dismay, this morning, just before my talk, I heard two lectures describing events that took place in the second half of the 1900s. And again I realized that I was wrong. In desperation, in the last 15 minutes, I went back to an earlier and more noble draft of my talk. Unfortunately, by now, it is slightly schizophrenic and disorganized.
But levity aside, the fact is that I really did not know what would be of interest to professional historians. What appears in the following report is my guess — and with it a hope that I have touched on some topics of interest.

20.2 Subject

I start with the premise that general relativity (GR) in the late 1940s in the U.S. was in the doldrums$^1$. This view arose as my personal observation made as an undergraduate student at New York University. At the time, nobody seemed either interested in or knowledgeable about GR nor did I find it easy to get information about potential graduate schools that had GR programs. In fact the only school I did find was Syracuse University — which eventually became my academic home for five years. (Jean Eisenstaedt (1) recently pointed out to me a paper of his that gives a much more complete and objective description of this stagnation of the field during this period.)

There are several points that I would like to try to convey to you. The main one being that in the early 1950s, we saw the beginnings of a reawakening of the field — in retrospect one can see that it occurred almost simultaneously in the several different schools (to be discussed below) — but in my inexperienced eyes I only saw it occurring at Syracuse University. The emphasis in my talk will be on the GR group, at Syracuse, under Peter G. Bergmann during the years (1950–61). I will describe the personnel there, a bit of their subsequent careers and some of the external interactions. The second (closely associated) point to be made was the remarkable number and high level of the collaborations that developed during this renaissance, between the different schools from around the world — and the total absence (as far as I could see) of professional jealousies or conflicts. I believe that the very high level of scientific activity in the different groups with the subsequent interactions between groups played a critical role in the rebirth of interest in GR. A prime example of this, (I emphasize and describe in more detail later), is that in the brief period, 1960–62, essentially, the entire theory of gravitational radiation was developed by the strong interaction of many workers from Syracuse, London, Hamburg and Warsaw via personal contacts and word of mouth communication. The published material came afterwards with, to the best of my knowledge, complete attributions and acknowledgments. Though there is no way to prove or document it, I am quite convinced that the high quality of the science came, at least partially, from this free exchange of ideas.

Though Bergmann was deeply involved in many different research projects (e.g., quantum gravity and the search for observables, gravitational radiation and statistical mechanics; see below) the main emphasis here will be on the development of radiation theory. The quantum gravity aspects of the Syracuse program are better known and have been already reported on (2).