Gravitational interferometers in Italy 1976: a first timid attempt. And a missed opportunity

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Abstract: In 1976, about 10 years before the conception of the Virgo project, two young undergraduates walked into the G23 lab of the Institute of Physics in Rome University – where Edoardo Amaldi and Guido Pizzella were leading the effort to build and operate the first small scale prototypes of cryogenic resonant gravitational wave detectors – applying for a thesis. Pizzella and Amaldi suggested studying the proposals for an interferometric detector, and possibly developing a benchtop prototype. The two theses got started, virtually with no supervision, as there was no expertise in optics in the group. We shall describe the thesis work, the prototype that was built and how the project was finally abandoned after the graduation of the two students (one of them is co-author of this paper).

Keywords: Interferometric detector, gravitational waves, Edoardo Amaldi, Guido Pizzella.

1. Introduction: The Italian and international context

This article aims at recounting a small, almost insignificant episode of the 1970s that could have opened the way to the search of gravitational waves (GWs) with interferometric techniques in Italy, but failed to do so. It is the story of two undergraduates looking for a challenging *laurea* thesis in the gravitational group founded by Guido Pizzella and Edoardo Amaldi at the Institute of Physics of the University of Rome (now Physics Department of Sapienza University of Rome).

The historical interest of this episode lays in the special environment in which it developed: the early years of the first Italian group working on GW detection, a group that had a strong influence on the development of the field in Italy in the following years, and which played an important role in the International GW community.[1]

We begin with a brief overview of the state of the art of GW research in the fall of 1975, when the thesis work of the two undergraduate students Livio Narici and Massimo Bassan began.

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[1] See also the paper by L. Milano and A. La Rana in these proceedings.

1.1. Year 1975: Gravitational Wave experimental research in the world

The research on gravitational waves had started in the late 1950s with the pioneering work of Joseph Weber, who computed the interaction of a solid bar with GW and the details of how to detect the small strain produced. In the 1960s Weber had claimed detection (Weber 1969), and this spurred a host of similar experiments (Aluminum bars suspended in vacuum at room temperature): Frascati-Monaco (Max Planck Institute), Argonne-Maryland, Bell Labs, Moscow University, Meudon Observatory (Paris), Geneva University. All these room-temperature resonant detectors – some working in coincidence at a large distance one from the other – collected data without confirming Weber's claim. With the aim of removing all doubts and ambiguities, and hopefully detect GW, W.M. Fairbank of Stanford University had proposed, in 1969, an experiment a thousand times more sensitive: a task which could be achieved by cooling the large bars to very low temperatures and by equipping the readout with superconducting electronics. This challenge was accepted by W.O. Hamilton, of Lousiana State University, and by Guido Pizzella in Rome. At the very beginning, Fairbank's ambitious project was to have a 5 ton bar cooled to 3 mK operating in about a year, but he was extremely optimistic: it actually took 11 years to cool the Stanford bar to 4 K, and 10 more years to cool Nautilus (a 2.4 ton bar) to 0.1 K!

1.2. Year 1975: Gravitational Wave experimental research in Italy

The experimental activity in Rome started in 1970 (Pizzella 2016), catalyzed by Pizzella and supported by Amaldi, who was at the time 62 years old. Besides them, Ivo Modena (cryogenic expert), Massimo Cerdonio (superconductive readout) and Renzo Marconero (electronics) composed the initial group. Located in the SNAM-Progetti Lab in Monterotondo – a research center funded by ENI – the Roman team engaged in setting up a large cryostat, to host a 3 m, 5 tons Al bar. In 1975, when our story begins, the cryostat had just suffered a serious accident, bringing the program to a halt; it would be resumed in 1982 at CERN, with EXPLORER. Therefore, Pizzella had decided to restart with a smaller project: a medium size cryostat (for a 300 kg, 1.5 m bar), to be built in the CNR "Laboratorio Plasma Spazio" (LPS, now IAPS-INAF) in Frascati. CNR was then the main financing agency of the project; INFN took over in the early 1980s. Umberto Giovanardi was coordinating this effort in Frascati, while most of the group, led by Guido Pizzella, was in the G23 lab of the University of Rome, experimenting on an even smaller cryogenic bar (30 cm in length):[2] Gianvittorio Pallottino and Franco Bordoni (electronics), Modena and Massimo Cerdonio.[3] A number of young post-docs were participating in the activity: Pasquale Carelli, Fulvio Ricci, Carlo Cosmelli, Sandro Barbanera, and several technicians: Eugenio Serrani,

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[2] The small bar is now displayed in the Department of Physics (new building) of Sapienza University of Rome.
[3] Cerdonio was on the verge of leaving for Trento, where he and his group eventually (early 1990s) built and operated an ultra-cryogenic detector, a twin of Nautilus.

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2. Thesis begins: interferometrists with no experience

From now on, we shall recount personal recollections of one of the authors (MB), and therefore switch to first person writing.

2.1. One thesis for two: We were the experiment

In the summer of 1975, Guido Pizzella gave a colloquium in the Institute of Physics, presenting the group and the research that was beginning at the University of Rome. Livio Narici and I were at that time two fourth year students, with good grades and looking for a thesis: we were fascinated by Guido's talk and by the challenge, technological but mostly scientific, that the search for GWs was offering. We were friends, but we independently went to Pizzella to ask for a thesis topic. Guido thought it through and proposed to join our efforts and look into this "new idea of detecting GW with laser light". He made it clear that none in the group had any experience about interferometry, nor even optics at large, and that we would be on our own: for this reason, he was assigning the project to both of us, an unusual procedure, as theses normally were (and still are) a one student task. In this respect, therefore, *we* were an experiment: how would two students, that had proved good in passing exams but with no practical skill, perform in such a situation?

We sought advice from experienced scientists. Dr. Franco Bordoni, then a CNR researcher in the group, was willing to help, but suffered health problems and was not around for long periods. Prof. Francesco De Martini offered his supervision, and his quantum optics group seemed a good resource; however, he was busy in getting his lab started and was not easily available. Besides, because of the incompatible personalities of our two reference persons, we soon realized we had to choose only one mentor.

2.2. First, read the literature!

The starting point was obviously to read the few available papers on the subject. At the time, the article by the Russian scientists Gertsenshtein and Pustovoit (1962) was not known in the West. The first documented experiment with a multi-bounce interferometer,[4] in the framework of GW research, is in a paper by Forward, Miller and Moss (1971). We read the paper over and over, coming to terms with mysterious concepts like "SNR" or "Power Spectrum", until we got an idea of what needed to be

[4] In a multi-bounce interferometer, the optical path of each arm is increased by letting the light bounce multiple times between the mirrors before recombining.

replicated. I would like to mention that in a footnote of that paper we read: “To our knowledge, the first suggestion was made by J. Weber in a telephone conversation with one of us (RLF) on 14 September 1964”, thus attributing to Weber the (Western) paternity also of this kind of detector.

We learned then that two research activities on interferometry for GW detection were underway in Europe: one at the Max Planck Institute (MPI) in Garching (near Munich), with a three meter interferometer in vacuum; and one in Glasgow University, where interferometry was used to monitor vibrations, or rather relative motion, between a pair of resonant bars.

2.3. A turning point: the Pavia Conference

In the fall of 1976, a cornerstone conference took place: the International Symposium on Experimental Gravitation, organized in Pavia by the Accademia Nazionale dei Lincei.[5] Despite our status of students, normally below threshold for such a trip, Prof. Pizzella provided us with the means of participating in the conference, our first presence on the international scene. There we had a chance to “interview” Dr. William Winkler of MPI and ask him many technical questions. The proceedings of the Pavia conference, published the following year (when we were finally approaching graduation), were for several years the only references to the work of the teams in Garching (Winkler 1977) and Glasgow (Drever 1977).

Back to Rome, our main problem was the lack of competence and equipment for optics in our lab. We bought a steel square plate with a side of 1.2 m and several magnetic holders to mount our rudimental apparatus; we asked the machine shop of our Institute to design and make some stands for mirrors and an orientable holder for the laser. The fabrication of the components took a long time, diluting our effort.

2.4. A trip to the Max Planck Institute in Munich

Bordoni proposed that we should visit the interferometer lab of the MPI in Garching. Pizzella agreed to defray the costs of the trip, and three of us left, by train, in full winter. The visit to the MPI lab was interesting and frustrating at the same time: we saw their 3 m interferometer, in vacuum, a lab full of dedicated equipment and a group of few, very competent researchers. We were also received by their group leader, Prof. Heinz Billing,[6] already an iconic figure in German physics.

The efficiency of the German lab made us feel frustrated as, by comparison, we realized that in our “do-it-yourself”, amateurish approach, we would never go very far!

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[5] The organizing committee was composed by Beniamino Segre, Edoardo Amaldi, Carlo Cattaneo, Giuseppe Colombo, Vincenzo De Sabbata, Livio Gratton, Gleb Wataghin and, in the role of scientific secretary, Bruno Bertotti, who was professor in the University of Pavia.

[6] Prof. Billing recently passed away on Jan 4, 2017, at the age of 102.

2.5. Rainer Weiss's Report: "The Bible"

In our trip to Munich, we learned about the existence of a precious internal report of MIT, written by Rainer (Ray) Weiss, where the signal and (almost) all the sources of noise of an interferometer were analyzed (Weiss 1972).

Back in Rome, we started looking for the valuable reference that, being an internal report, was not to be found in the libraries we had access to (Institute of Physics, CNR, LNF). We mentioned the problem to Prof. Amaldi, who was always very interested in our activity and eager to help. He reacted to our request immediately: he had been receiving the MIT quarterly reports since ever, and it was just a matter of locating that particular issue. We followed him in a small, dark room crammed with piles of papers, where he began to sift through disordered heaps of documents, raising dust (that did not bother him) and finally resurfacing with the famous issue 105… We had it! Indeed, that report was extremely useful for us, and for many more after then: the LIGO people still refer to it as "the Bible", and they have edited an online reprint [Weiss report], to make it available to everyone, as it is still considered today a cornerstone in the field.

3. Student's life: a few anecdotes

Prof. Edoardo Amaldi was very interested in our thesis work. He told us once: "As a very old spectroscopist, I like to hear about optics". About half way through our theses, he summoned us for a briefing; we were asked to give a short status report on our activity. I remember that we were in awe when we walked into the *sala lauree* (convocation hall) of the Institute, to deliver our first seminar ever. Amaldi was still enormously influent in Italian physics at the time: so, it often happened that people expressed great interest in what we were doing, not so much because they really cared about interferometric GW detectors, rather as a way to approach Amaldi. We learned it *the hard way*, after many unfulfilled promises of collaboration. Still a year or two after graduating, we were invited to the INFN labs in Frascati to discuss possible applications of a free electron laser to GW detection!

Among the goals of our project, there was the construction of optical delay lines (Herriott, Kogelnik, Kompfner 1964): two spherical mirrors in almost confocal configuration, where the light bounces back and forth many times before recombining, thus extending the optical path (and increasing the sensitivity to GWs).[7] In order to feed the laser beam into the delay line, one must have a hole in one mirror: through the same hole, the light is then extracted, after many bounces. The task may seem a trivial matter, but was not. After a broad search, I landed on a post-doc in Engineering who had the know-how and the machine to drill holes in glass: so, after spending several days operating an ultrasonic drill with a specially machined hollow tool bit and using a lot of patience, I managed to produce a hole in two of our precious, spherical mirrors.

[7] The so-called *Herriot optical delay lines* were later substituted, in large interferometers, by Fabry-Perot cavities, that proved to be more advantageous in several respects.

When the delay lines were finally assembled, they apparently violated the geometric rules that predict the number of bounces of the laser light, given the focal length and distance. After much testing and head scratching, we finally realized that our mirrors had a wrong curvature: 84.5 cm, while we had asked for 80 cm. The *Officine di Precisione dell'Esercito* (Army precision machine shops) had sold us what they had in stock, without bothering to warn us. Nevertheless, we assembled interesting configurations, with a temporary gold coating. Afterwards, we sent the mirrors to the *Laboratorio Italiano Strati Sottili* in Florence, to be coated with a dielectric-reflecting multilayer… but, we left before the mirrors returned.

By reading the literature, we convinced ourselves that a single-longitudinal-mode laser was needed, because spectral purity affects the frequency noise. Clearly, the occurrence of observing frequency noise was far beyond the most optimistic goal we could have for our project. Although it can appear obvious now, even at that time we were aware that many were the problems we had to face before frequency noise would become relevant. However, we set out and looked for a single mode laser. It turned out that Prof. Renato Cialdea, working in the lab next door, had a Spectra-Physics 119 laser that, despite a mere 100 μW of output power, had the required spectrum. Only a little detail needed to be solved: the laser was no longer working, "probably due to a loss of the He-Ne gas in the cavity". Therefore, I bravely turned to the LNF labs of INFN, where a friend and colleague of Pizzella had offered his help. There was a technician quite skilled in glass blowing, who could manage to open the bulb containing the gas mixture and the optical cavity, and to replenish it with "some mixture of gas". I landed in Frascati labs in the middle of a trade-union struggle for a contract negotiation. At the time the technicians were often on strike, and when they were not, they would gather to discuss "over coffee" for hours. I received very little attention but, at the end of many days of begging and waiting, the bulb was finally replenished. Needless to say, the device never lased again, and we finished our theses with a brutal SP155, a laser more intended for alignment than for precision experiments.

In the mid of the 1970s, the Crab pulsar, discovered in 1968 and rotating with a frequency of 30 Hz, was considered a very promising source of GWs. However, the emission frequency (60 Hz, twice the rotation frequency), was too low to be addressed by a resonant bar, that would need a length of about 41 m.[8] Pizzella was not easily discouraged and conceived a long resonant bar to be assembled in space (to avoid the seismic noise, too large at low frequencies). He asked us to investigate if the long baseline could benefit from an optical readout. We dedicated the last chapter of our theses to this issue, analyzing the possible advantages and main obstacles. In order to prove the concept, the group technician was asked to try to fabricate a cylinder, by fastening together three shorter sections, through a 100 mm thread on the bar axis. However, due to the large size of the Al thread, this fastening invariably and irreversibly seized up: poor Mr. Serrani had to restart the effort many times over, until

[8] The length of a resonant bar is related to the frequency of the emission frequency and the speed of sound by the equation: $L = V/2f$.

he finally succeeded: but the composite antenna resonated with a very low Q value and the project was set aside.

4. What we managed to do

During the period we spent working on our thesis project – roughly a year and a half – we were able to assemble a "toy interferometer". We borrowed some optical mounts, others were designed and built in the machine shop; we bought some x-y micro-positioners to align two delay lines at 90°, a beam splitter and a receiving photodiode. The optical bench was a 1.2 x 1.2 m^2 steel base, resting on a bed of springs that was supposed to provide some vibration isolation. The table supporting the plate was built of bricks in the lab basement. We played for some time with various configurations of the Herriott delay lines, varying the number of bounces and the pattern of the reflecting spots on the mirrors (see Figure 1). Particularly popular with our colleagues were the Lissajous figures we were able to produce. Modena kindly provided smoke from his pipe to make the light beam visible, so irrespective we were of the quality of the reflecting surfaces! Finally we settled on a configuration with an arm length L = 80 cm, providing about 50 bounces. We used this set-up to measure the reflectivity of our gold-plated mirrors and the responsivity of the photodiode. We also measured the noise spectrum of our "generic" laser, finding, above 15 kHz, a good agreement with the expected shot noise. In 1977, spectrum analyzers in the acoustic band were not yet available (at least, not in our lab). Therefore, a spectrum was produced with a lock-in, by changing the reference frequency and integrating the noise on many points per decade: a task requiring hours!

We did not even get close to trying to lock the interferometer in the dark fringe, using a modulation scheme that seemed, at the time and to us, very innovative. We had ordered two Pockel cells, electro-optic modulators that we had learned to be essential to lock the interferometer. It took about one year to have them ordered, shipped and custom cleared: when they became available, we had already graduated and moved on.

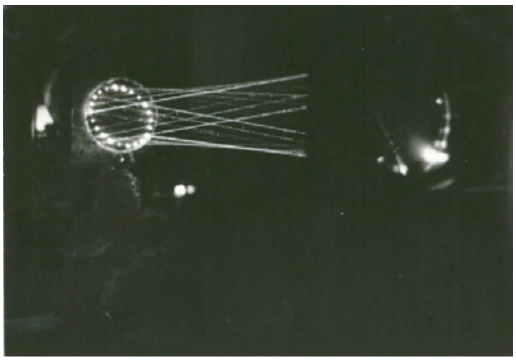

Fig. 1. One of our Herriott delay lines: the envelope of the laser beams, made visible by Moden's pipe smoke, describes a hyperboloid.

5. What happened afterwards... And why

We were very honored when Prof. Amaldi proposed to be a co-advisor of both our theses, even if this implied the unpleasant task of asking Franco Bordoni to step aside. To our knowledge, we were the last *laureandi* of Amaldi in his long academic life.

Our last effort was to convince our *contro-relatore* (opponent reader), Prof. G. Baroni, that the two theses we had submitted were actually different, and the product of individual work. Separately summoned by her, we both replied: “I cannot tell what is different in our theses, because I have not read the other”.

Eventually, despite the turmoil and the violence of the spring 1977, that caused the University to shut down for weeks, and forced us away from the lab, we graduated *cum laude* on July 27. My friend Livio Narici left for his military service while I, waiting for a call to the army (that fortunately never arrived), spent one year teaching in schools and visiting the lab, trying to complete some of the projects we had left unfinished at graduation. Our effort had produced little original, worth publishing work, but, following a suggestion of our advisor Pizzella, we wrote an internal report, summarizing the state of the art of our project (Bassan 1978).

Two years later (1979), we both left Italy to pursue a PhD in the USA, joining groups working on resonant bars, at Stanford University (MB) and at Rochester University (LN).[9] The small interferometer in the G23 basement lab was abandoned, no follow-up to our theses took place: our work left no legacy; it was just a probe for a technology then considered “not mature”.

In this respect, it was a missed opportunity: Italian science could have started investing in interferometers about ten years earlier than it did. We may suggest two main reasons for this giving up. On one hand, we had not reached a performance level worth investing new energies – we did not go beyond an “amateurish” effort, and had not proven the technology to be viable. On the other hand, the Amaldi-Pizzella group was under pressure to set up cryogenic bars, a frontier project as well, developed in collaboration/competition with the groups of Stanford and LSU. They chose to keep focused on the bar technology, a choice that payed off in the short-medium term. Explorer and Nautilus, the two resonant detectors that the group built in the 80s and operated up to the 2010s, represented for long time the cutting edge of GW experimental research.

Then, in 1985, a fresh interferometric start: A. Giazotto and A. Brillet gave birth to a new effort, which soon evolved in the project Virgo. They managed to set up a large, international enterprise, attracting scientists from high-energy physics. They played in a different league! However, this is a story for the next paper.

Acknowledgments. The authors thank Prof. Guido Pizzella, the thesis advisor, and Prof. Livio Narici, the co-protagonist of this story, for reading the manuscript and providing suggestions.

[9] Just as it happens now, no positions nor fellowships were available in Italy: the whole academic hiring process was on hold, waiting for the law that, in 1980, reformed the University system.

Professor Ivo Modena passed away during the writing of this manuscript, on February 4, 2017. We would like to dedicate this paper to his memory.

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