'Out of the dusty labs'. Really ? Bell Labs, the transistor and the myth of isolated research

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« *Out of the dusty labs* ». Really ?

Bell Labs, the transistor and the myth of isolated research (1925-1960)

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> "*The current skepticism about basic research in industry imagines a world of detached corportate laboratory that existed only for a moment in a much longer history of successful balancing of the commitment to the long term with the need for a commercial payoff. This historical misperception perpetuates the error that basic research is a luxury firms cannot afford. Reducing the scope of innovation in this way is a recipe for reducing innovation, period.*" (Lipartito, 2009, p. 153-154)

1. Introduction

The role of corporate research and central research laboratories[1] has always been an important, and frequently controversial, subject in management science. Indeed *Why do firms do basic research (with their own money)* ? (Rosenberg, 1990) has been and remains a fundamental strategic question for academics, managers and policy makers. And actually we've seen important evolutions in the role of corporate research, from the apex of the 70's to the *research blood bath* (Buderi, 2000) of the 90's and the current skepticism on the relevance of corporate research. This declining role of corporate laboratories over the last 40 years is now well documented (Hounshell, 1996 ; Mowery, 2009 : Arora & al., 2018). It has been notably (but not only) attributed to an isolation of research from corporate and business unit's strategy (e.g. Graham, 1986 & 2017). This paved the way for more « open » approaches to innovation processes (Chesbrough, 2003 ; Chesbrough & al. 2014). One of the consequences of this evolution has been a shift of academic research toward the analysis of interfirm organizations and networks to the detriment of the internal organization of research (Powell & al., 1996; Argyres & Silverman, 2004). This leaves us with the picture of corporate research as « dusty labs » (The Economists, 2007) that were « ivory towers » in which scientists defined their own objectives, most of the times loosely related to corporate strategy (Buderi, 2000). This perception of corporate research has been accentuated by the popular managerial literature. For example, the widely cited book *Third Generation R&D* by Roussel & al. (1991), that paved the way for 4, 5 or 6th generation (e.g. Nobelius, 2004), depicted the 50's and 60's, considered by some as the golden age of corporate research, as characterized

[1] In this paper with discuss specifically the role of research, as distinct from developpement. In order to simpfily let's say that Research focuses on the creation of new knowledge, whereas development takes this knowledge to the product launch phase through engineering.

by a « *strategy of hope* » in terms of management, i.e. investment on basic research that may (or not, with some luck) leads to breakthrough products. This view has gained momentum at the beginning of the 2000. And actually, behind the discourse, Arora & al. (2018) have documented a decline in the scientific content of industrial research, largely explained by the reluctance of large corporation to invest in research.

In this paper we want to question this story. Were the famous corporate research laboratories really the « ivory towers » depicted in the literature ? Incidentally, what are the conditions that makes corporate research strategically relevant ? Aren't we victim, as suggested by Lipartito (2009), of an historical misperception of the role of corporate labs ?

To do this we adopt a qualitative methodology. We propose to revisit this question through an historical case study of Bell Laboratories. In particular we will focus on the post-war period and the invention of the transistor, which represent an archetypal success of corporate research leading to a breakthrough technology and the 1956 Nobel Prize in physics.

In so doing we will demonstrate that, contrary to a widely shared belief in the managerial literature, Bell Labs were completely integrated in ATT's strategy and part of carefully designed innovation process organizing a rapid transition from research to development and manufacturing, while simultaneously allowing the pursuit of the research effort. Thereby two different transistor design were invented at Bell Labs in 1947-48.

The paper is organized as follow. In the first part we present the evolution of corporate research and poses the research question. Section 3 present the methodology. The Bell Lab Case and the transistor story are presented in section 4. Section 5 discuss the case in the light of corporate research management. A conclusion summarizes the results.

2. The evolution of corporate research : an overview.

The history of corporate research is well documented in business history and innovation management (see Hounshell, 1996 ; Mowery, 2009 ; Lecuyer, 2015 ; Graham, 2017), at least in the US case. In order to set the context of our reflection we can identify four main phases :

1. Corporate research as we know it appears in the US in the early 20th century, inspired by the german model, when firms like GE, DuPont, Westinghouse, Kodak or ATT created labs to internalize research in order, first, to control production processes and quality and, rapidly, to foster innovation. This leads to a tremendous growth of the number of research labs, from 50 in 1913 to 2000 in 1940 (Lecuyer, 2015). This widespread investment in research was justified by still emblematic successes like tunsgten filament lamps at GE (1909), vacuum tubes at Bell Labs (1912) and, probably the most famous, Nylon at DuPont (1935, see the classic from Hounshell & Smith, 1988).

2. But this was only the beginning. The strategic role of research was indeed greatly amplified by the 2nd world war. The fundamental role played by research during the conflict (see Buderi, 1996 for the radar project and Rhodes, 1986 for the atomic bomb/manhattan project), its legitimization by V. Bush famous *Science : the endless frontier* (1945) report[2], and the beginning of the Cold War opens the era of Cold War Science. The number of researchs labs exploded to 11000 in 1975 (Lécuyer 2015), following the massive impetus of defence-related spendings which, for example, represented 80% of federal R&D spendings in the 50's. This era was also marked by tremendous successes, typically the transistor (see below) or the great space-military projects (Atlas/Titan, Polaris, SAGE, Apollo, etc.).

3. However things begin to change in the late 60's – early 70's. First, the weight of military spendings and the prestige of physics pushes toward more fundamental research and created a gap with business goals at a time where US competitors, first and foremost Japan, focuses on product development performance. Second the economic crisis of the 70's lead firms to a relative decrease of federal R&D spendings and a fundamental questionning of the firm's organization. The crisis of the large diversified corporation and the move toward a recentering on « core business » leads to outsourcing of many activities and had profound consequences for research. Indeed some studies began to question the relevance of basic research (project hindsight, 1969) and firms starts to diversify their sources of new ideas. In particular they began to rely more on academic research, whose role has grown with cold war science. In this perspective the 23 million dollars grant from Monsanto to Harvard in 1974 is considered a turning point. The logical consequence of this move was the cut in firms R&D budget which culminated in the *research bloodbath* of the early 90's when many laboratories were abruptly closed (see Buderi, 2000).

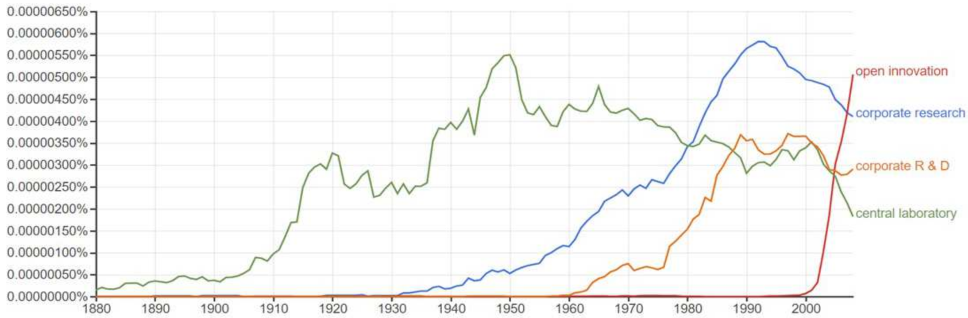
4. This evolution was, in our view, theorized by H. Chesbrough in his famous *Open Innovation* (2003). He describes and analyzes the tranformation of research from a closed innovation model based on large central laboratories (mainly Xerox Parc in the book) to an « open » one where the firms largely relies on outside source of knowledge during the innovation process. This correspond to the tremendous growth of academic-industry partnership, interfirm cooperation, venture capital or the rise of consortium like sematech for semiconductors (1987). Biotech and ICT constitutes the paradigmatic cases of this evolution (for biotech see Powell, 1996 ; Pisano, 2010). Wether this open innovation model is really new is a question that we leave open to debates (see Mowery, 2009 and Chesbrough & Bogers 2014 reply).

[2] Criticizing Bush report is outside the scope of this paper. Let's just say that we agree with Naraynamurti & Odumosu (2016) when they point to a misunderstanding by Bush of the causes of the radar project success which as much basic research as it was engineering.

This (roughly summarized) evolution has profound implication for the research role and management and for its place in academic research.Three points are worth noting

1. First the decline of corporate research (Buderi, 2000 provides an excellent overview) leads to a corresponding decrease of firm's investment in science, R&D being more concerned by the Development and practical application than by the Research, i.e. the creation of new knowledge (Arora & al, 2018).

2. This leads to a corresponding decline in the academic interest for Research as a specific function distinct from Development. During this period we observe a shift of academic research toward the analysis of interfirm organizations and networks to the detriment of the internal organization of research (Powell & al., 1996; Argyres & Silverman, 2004). Indeed, whereas dozens of papers deals with networks, firms cooperation or industry-academia partnership, the role of corporate research is fading out of view. This trend is quite clear in the following graph from Google Ngram which mirrored the evolution described above.



In this perspective it is striking to note that contemporary handbooks of innovation management does not deal with research. For example there is nothing on this topic in the *Oxford Handbook of Innovation management* (Dodgson & al, 2014) or in the *Handbook of New Product Development* (Loch & Kavadias, 2008). Even the standard *Strategic Management of Technology and Innovation* (Burgelman & al, 5th edition, 2008) comprise only 3 papers on this question (and quite old one) on the 88 papers composing the book. A complete literature review remains to be done but a first search in academic journals (CIM, Research Policy, ICC, JPIM, SMJ in particular) leads us to hypothesize that this shift toward open innovation is also present in peer-reviewed journals.

3. The trend is even clearer in the economic press which eagerly celebrates the end of central R&D in the early 2000's. An complete overview is impossible but the trend is clear in articles such as « *Is the Central R&D Lab Obsolete?* » (MIT Technology Review, april 24, 2001), « *Why Big Companies Can't Invent* ? », (MIT Technology Review, may 1, 2004) « *Corporate Labs Disappear. Academia Steps In* ». (New-York Times dec 16,

2007) or « *The rise and fall of corporate R&D. Out of the dusty labs*. » (The Economist, Mars 2007).

This historical overview leave us with the picture of corporate labs that were or became « ivory towers » in which scientists defined their own objectives, most of the times loosely related to corporate strategy (the so-called « strategy of hope » of the 50'S - 60's in Roussel & al., 1991). In this perspective investing in R&D is « fatalistic » i.e. mainly a cost that sometimes pay off with a bit of luck… This is reinforced by the focus of the literature on famous failures of this labs, for example RCA's Videodisc crash (Graham, 1986) or the inability of Xerox to commercialize the invention of its much celebrated Xerox Park, most notably the personal computer (Smith & Alexander, 1999 ; Hiltzik, 1999 ; opening chapter of Chesbrough, 2003)[3].

In this paper we want to question this perception of the role corporate research. Indeed we can wonder if the famous corporate research laboratories really were the « ivory towers » depicted in the contemporary literature ? Incidentally, this should lead us to discuss the conditions that makes corporate research strategically relevant. This is all the more important that, as pointed out by Graham (2017), the organization of research remains an important managerial question and its decline over the last decades may only be a a period in history. Important firms (Apple, Google…) are still investing in corporate research. In this perspective this work may yield important insight for the future management of Research.

3. Research methodology

Our approach is qualitative. We propose to revisit this question through an historical case study of Bell Laboratories during M.J. Kelly's tenure. More specifically we will focus on the invention of the transistor (roughly from the mid-30's to the mid-50's), which represent an archetypal success of corporate research leading to a breakthrough technology and the 1956 Nobel Prize in physics. Indeed we think that there is a lack of micro-analysis of the *internal* organization of research in the contemporary academic literature. Most of the (too rare) remaining literature on this question frequently relies on quantitative methods, for example to assess the relevance of different structural configurations (e.g. Arora & al., 2014). By focusing on a case of the post-war period, described as the « strategy of hope » phase, we want to question what we consider the myth of isolated research.

[3] This focus on the Xerox PARC is actually questionable. Indeed the PARC was 1) created quite late in this story (1970) ; 2) from scratch by hiring people outside of Xerox ; 3) didn't play any role in Xerox history (contrary to its Webster research center) before 1970. Therefore the PARC is not representative of the historical corporate research centers like Bell Labs, for example (created in 1912 and in 1925 as a subisidiary).

To do this, we rely on the historiography of Bell Labs and the transistor which, as far as we know, as not been fully exploited in economics and management research since R. Nelson's 1962 paper. Three sources are important for our purpose :

1) history of Bell Labs in general (Lipartito, 2009 ; Gertner, 2012) ;
2) history of the transistor (Nelson, 1962 ; but first and foremost Riordan & Hoddeson, 1997) ;
3) testimony of the actors involved in the process, in particular W. Shockley (1956, 1974 & 1976), J.A. Morton (1970) and Bell Lab President, M.J. Kelly (1943 & 1950). This testimonies are of particular significance since they question the common discourse of research as an « ivory tower » and the classic basic/applied distinction. This is particularly true of M. Kelly, Bell Labs President until 1959, who spent most his 1950 adress to discuss the systemic dimension of innovation and the corresponding Bell Labs organization.

This leads us to construct a case study of the transistor which is presented in the next section.

4. Bell Labs and the invention of the transistor

What were Bell Labs ? A short overview

Our goal here is not to propose a complete history of Bell Laboratories which would be outside the scope of this paper (for an excellent overview see Gertner, 2012). More simply we want to clarify what Bell Labs really were. Indeed, because of their outstanding successes (in particular 8 Nobel Prizes), they are frequently assimilated to a basic research organization. But this is wrong. Created in 1911 and formally as a subsidiary of ATT in 1925, Bell Labs first major success was, in 1912, the improvement of the vacuum tubes originally invented by Lee de Forest in 1906 that play a fundamental role to amplifiy the signal in long-distance communication, a strategic activity for ATT. This first success leads ATT to transform Bell Labs into a subsidiary in 1925. But their role has never been limited to research. They were a true research <u>and development</u> organization. They were thus in charge of research and engineering. And, when you look at the figures, research was actually a minor part of the activity of the 10 000 researchers and engineers, approximately 15% (Morton, 1972), the rest being devoted to system engineering (15%) and development (70% : 20% fundamental development / 50% specific development design). Therefore they took in charge the development of the new technologies and of the equipment necessary to « improve communication » in the huge ATT network, from customers terminal to submarine cable to long-distance amplifier, etc. In order to do so, they created carefully designed processes to manage the transition from research to fundamental development to specific development to implementation at Western Electric, the manufacturing arms of ATT, and in the Bell Divisions. It is thefore misleading to assimilate the Bell Laboratories to a research organization. They were much more than that.

Inventing the transistor at Bell Labs[4]

The story of the invention of the transistor at Bell Labs starts in this context at the beginning of the 30's. At this date Mervin J. Kelly[5], Director of research, was well aware of the fundamental limitations of vacuum tubes, which played a fundamental role in telephone networks. It was clear at this date that vacuum tubes were fragile, hard to manufacture, power hungry and that they produce too much heat. Given their role as amplifier or electronic switches, this constitute a strategic concern for BL and ATT. One of the avenue identified by M. Kelly to overcome these weaknesses was the exploration of the potential of semiconductors (SC)[6]. The problem was that semiconductors physics was a nascent field at this time. Even if they were currently in use in some electronic equipment like radio and radar, semiconductors were hard to produce, difficult to experiment with and the theoretical knowledge of their inner functioning was very limited. Quantum physics, itself a new theoretical field in physics, has provided insights into their behavior but on very limited type of semicondictor (copper oxyde) and this remain a domain of fundamental research.

Given the novelty of the field, exploration at Bell Labs starts in a very academic manner. In 1936 M. Kelly decided to hire new brilliant PhD in physics to study the potential of SC for ATT, in particular the future Nobel prize winner William Shockley. A seminar was organized at BL to diffuse the knowledge of quantum physics among the scientists. W. Brattain, co-winner of the 1956 nobel prize and a famous experimenter was among them. This leads to the first experiments which tried to copy the structure of vacuum tubes in SC. The first try in copper oxyde in 1939 was a failure.

Word War II abruptly stops this research at BL. However the organization became deeply involved in a project that will have a major impact on the field of SC : the radar project (see Buderi, 1996). Indeed vacuum tubes are too big and fragile for radars. Therefore they will be replaced by SC, in particular Germanium and Silicon. The war effort thus leads to tremendous improvements in the manufacturing of these SC. It also demonstrates the extraordinary impact of interdisciplinary research organized by project (Galison, 1997).

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[4] This section is based on an ongoing work with L. Petitgirard (CNAM). See Lenfle & Petitgirard, 2018. The fundamental reference on this story are Hoddeson, 1981 and Riordan & Hoddeson, 1997.

[5] Mervin J. Kelly was Director of Research for 1936–1944, Executive Vice-President for 1944–1951, and President of Bell Labs for 1951–1959.

[6] « *A semiconductor material has an electrical conductivity value falling between that of a metal, like copper, gold, etc. and an insulator, such as glass. Its resistance decreases as its temperature increases, which is behaviour opposite to that of a metal. Its conducting properties may be altered in useful ways by the deliberate, controlled introduction of impurities ("doping") into the crystal structure. (…) Some examples of semiconductors are silicon, germanium, and gallium arsenide. (…) Semiconductor devices can display a range of useful properties such as passing current more easily in one direction than the other, showing variable resistance, and sensitivity to light or heat. Because the electrical properties of a semiconductor material can be modified by doping, or by the application of electrical fields or light, devices made from semiconductors can be used for amplification, switching, and energy conversion.* » Source : https://en.wikipedia.org/wiki/Semiconductor

Rich from this experience, research starts again at BL in april 1945. W. Shockley & W. Brattain, back from their war duties, experiments again with a new design based on the so-called « field effect ». This was a new failure. But soon the organization changed. From his own experience of the war, specifically the radar project, M. Kelly decided to create a multidisciplinary team to study solid-state physics. Under the leadership of Shockley, the team comprises experimenters, theoreticians, chemists, electronician, etc. They were soon co-located on BL new campus build at Murray Hill, NJ. They were driven by the goal defined by Kelly in the mid-30's : design of amplifier in SC for applications in ATT's telephone network. Their first decision, based on the knowkedge gained during the conflict, was to focus the research on germanium and silicon. Indeed, the radar project had demonstrated their potential, even if they were not well understood theoretically. Simultaneously the SC team continues to grow. October 1945 was thus marked by the arrival of J. Bardeen, who had just finished his PhD at Princeton on Quantum physics under the supervision of E. Wigner[7]. Shockley asked him to check the validity of his calculation to understand the failure of the april experiments. Bardeen confirm the calculation and his research leads him to propose, in march 1946, that the failure may be caused by a « surface states » effect that « trapped » the electron at the surface of the material.

The hypothesis was considered rich enough to lead Shockley to dedicate Bardeen & Brattain to a systematic exploration of the way to bypass surface states. The theoretician / experimenter duo then fully exploit the potentiel of the SC group to study a vast array of potential possibilities : doping, photovoltaic effect, magnetism, temperature, etc. 18 months later, in november 1947, Bardeen & Brattain confirms the surface states effect and show how to overcome them by using electrolyte.

This, according to Shockley (1974), marks the beginning of the *magic month* which will lead, in a burst of inventive scientific activity, to the invention of the transistor. Actually Bardeen & Brattain continue their work. Brattain suggestion to try the old « point contact » technique leads them to demonstrate a first amplification when the device is immersed in electrolyte (21/11/1947). It was too weak to be of any use, but it confirms the potential of this path. Then, on december 8, during a lunch conversation between Shockley, Brattain and Bardeen, the later suggest to switch from silicon, their material of choice, to the so-called « high back voltage germanium » produced at Purdue University during the war. They continue their test with different configurations (see Shockley, 1974 or Hoddeson, 1981 for a detailed presentation) until the famous december 16, 1947 experiment, during which they obtained a significant amplification. The first « transistor », called a « point-contact transistor », was born. It was announced publicly in june 1948.

However the story was far from over. The december 1947 device was a research prototype, very far from a working transistor. Furthermore, even if it works, the causes of the amplification were not understood by the team. Thus, in late 1947 – early 1948 the work goes

[7] Nobel Prize 1963.

in two direction. First a « fundamental development » group, under the leadership of Jack Morton, was created to design a working device based on the december experiment. Second, W. Shockley, frustrated by not being one of the inventor[8] of the new device, decided to continue the exploration of amplification in SC by studying another design for the transistor. Indeed he was not completely convinced by the hypothesis that amplification happens at the surface of the SC in the point-contact transistor. Furthemore he remains a strong proponent of a « junction » device composed of different « layers » of SC. His frantic, lonely, theoretical works quickly pay off since, in late january 1948, he proposed a new design, the junction transistor with a n-p-n structure, based on a new hypothesis concerning amplification in SC, « minority carrier injection »[9]. Then comes a fundamental experiment at Bell Labs. While exploring the functioning of the point-contact transistor, JN Shieves, member of the SC group, decided to test a design in which the two contacts were placed on both sides of the SC, instead of close to one another. To his great surprises, the device works. This has a direct consequence : amplification does not happen at the surface of the device, as suggested in particular by Bardeen. However, when he presents his results to the group, he explains that he had no alternative explanation. This is the moment chosen by Shockley to unveil to the stunned group his new theory that explains both the phenomena observed by Shieves and the potential of the junction transistor. It will took another two years, the transfer to « exploratory development », and fundamental breakthrough in the metallurgy of SC, to demonstrate the feasibility of a junction transistor.

The epilogue of the story is well-known (see Brown & MacDonald, 1982 ; Lecuyer, 2006). The point-contact transistor proved to be an industrial nightmare and had only limited application, mainly in the military. The junction transistor, on the contrary, becomes the foundation of the electronic revolution. W. Shockley directly triggered this revolution when he leaves BL to create his own firm, Shockley Semiconductor Laboratory (SSL), in Palo Alto in 1956, the year he receives the Nobel Prize in physics with Bardeen & Brattain. Even if his firms failed, he is « *the man who brought silicon to the valley* » and SSL was an extraordinary school for the future leaders of the semiconductor industry (G. Moore & R. Noce, typically) that created Fairchild Semiconductor in 1957, after leaving SSL. Here again military applications, first and foremost the Minuteman ICBM program, were key to the take off of this industry. On the ATT side, it will take more years to semiconductors to start replace vacuum tubes and mechanical

[8] « *Frankly Bardeens and Brattain's point-contact transistor provoked conflicting emotions in me. My elation with the group's success was balanced by not being one of the inventors.* » in W. Shockley, 1974, p. 54.

[9] « *Minority carrier injection, in electronics, a process taking place at the boundary between p-type and n-type semiconductor materials, used in some types of transistors. Each semiconductor material contains two types of freely moving charges: electrons (negative charges) and holes (positive charges). Electrons are the more abundant, or majority, carrier in* n-*type materials, holes being the less abundant, or minority, carrier. In* p-*type materials, however, holes are the majority carrier, and electrons the minority carrier. If a battery is properly connected to the semiconductor material, the* p-*type material may acquire additional electrons (minority carriers), injected into the* p-*type material from the* n-*type material by the flow of electrons from the battery. This is minority carrier injection. It is important in bipolar junction transistors, which are made of two* p-n *junctions.* » Source : https://www.britannica.com/technology/minority-carrier-injection

in the Bell System[10]. Computer will of course be the driving application of transistors but, legaly forbidden to enter this market for anti-trust reasons, ATT did not benefit from their extraordinary development.

5. Analysis : Science and corporate strategy at Bell Labs

The transistor case demonstrate that, contrary to a widely shared belief in the managerial literature, Bell Labs were very carefully integrated in ATT's strategy. It was not the result of a « strategy of hope » i.e. of a research decided by researcher themselves that, almost accidentally, leads to a breakthrough innovation. Quite the contrary. Indeed, as we have seen, the invention of the transistor is the result of a research effort launched in the mid 30's by M. Kelly on the basis of a thourough assessment of limits of current technology (vacuum tubes) and of ATT future challenges (high volume / high speed trafic). Moreover this commitment of the firm lasted over 10 years (15 if we consider the war period). In this perspective, as demonstrated by Lipartito (2009) M. Kelly follow the strategy of his predecessors, in particular F. Jewett, of a research organization carefully integrated in ATT strategy and processes. Indeed, from their creation, BL had developed processes to develop strong link with strategy on one side, and with manufacturing on the other. It has never been « isolated » from ATT. As explained by Lipartito,

> « *here was the lab strengh, « continuous operation » from research to application. Though personnel were free to pursue fundamental work, the labs was not set up « separate and apart » from daily operations of « commercial design and economic consideration », as were other research entities. Research and development department were in « close proximity », and information flowed between them casually and informally. The research worker served as a consultant to the development engineer, and researchers had a good understanding of the field operations of the apparatus they are working on* ». (2009, p. 144).

In this perspective it is very interesting to note that, in a 1943 memo entitled *A first Record of Thoughts Concerning an Important Postwar problem of the Bell Telephone Laboratories and Western Electric*[11], M. Kelly, now Director of research, discuss at lenght what he considers the main problem of BL i.e. … the link with manufacturing !! His concern was to smooth the transfer from research to production and to help the division move to the new electronic technologies developed at the lab. This leads to the creation of branch laboratories within the divisions. This trend was reinforced later by the creation of a department of system engineering at Bell Labs in 1955 in order to integrate their work in the ATT network[12]. This

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[10] The first application was in a card translator equipment in 1952 (Kelly, 1953). They appear in transatlantic cable as a substitute to vacuum tubes in… 1968 (Morton, 1970).

[11] We are very grateful to Dr. S. Hochheiser, from ATT Archives in Warren, NJ, for sending us this document.

[12] See also Kelly 1950 adree to the Royal Society in London in which in explain at lenght the integration of research in the overall innovation process. It is interesting to note the place granted to the physical organization of the lab to foster interdisciplinary cooperation and flexibility.

emphasis on the integration of research in ATT's innovation process, is clearly visible in the transistor case with the swift move from research to exploratory development. Research at Bell Labs was was part of a carefully designed innovation process organizing a rapid transition from research to development and manufacturing, while simultaneously allowing the pursuit of the research effort.

The invention of the transistor was also, a point rarely mentioned in the literature, strongly influenced by the new practices of research/engineering management developed in major word war II projects, such as the Radar (in which Bell Labs plays a major role) and Manhattan projects. It benefited from M. Kelly's reorganization of Bell Labs after the war, namely the creation of an interdisciplinary research group on semi-conductors, under the leadership of W. Shockley. Indeed interdisciplinarity foster a very rich, multidimensional, exploration process and facilitates rapid experimentation capability (see Thomke, 2003). The *magic month*, as Shockley called it, constitutes a spectular example of the relevance of this approach (see below). Here again research management plays a central role.

<u>*The magic month measured by scientist's notebook entries in Shockley (1974)*</u>

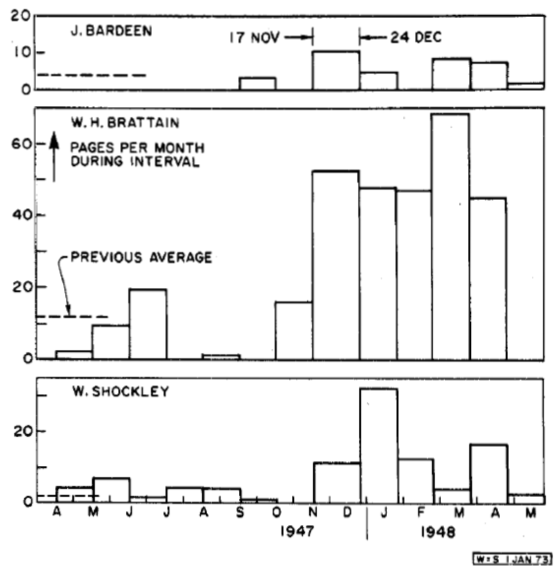

Fig. 3. The quantitative impact of the observation of 17 November 1947 on "the will to think" as shown by the number of notebook pages used per month. (Areas represent total pages used and ordinates represent rates. Each bar is one calendar month except for the one starting on 17 November 1947 which is lengthened to Christmas Eve 1947 to include the famous notebook entry of Walter H. Brattain reproduced in Fig. 17. After 25 April 1948, Brattain shared notebook use with an increased number of technical assistants; since the data then were not comparable to earlier data, they have been omitted.)

What is very interesting for our research question is to discuss the impact of this policy on the researchers themselves. Here the recollections of W. Shockley are very interesting. He is crystal clear that M. Kelly's policy had a major impact on his work and motivation right from the start. For example, in his 1976 recollection of the invention of the transistor (Shockley, 1976) he always make the link between his scientific accomplishments and Kelly's initial vision explainging that

> « *A key motivation that stimulated my will to think about transistors came from Dr. M. Kelly, who was then Director of Research at BL, a position he held before becoming President some years later. Dr Kelly visited me for the purpose of emphasizing his objective of introducing electronic switching in the telephone system. He said that he looked forward to the time when metal contacts, which were used in telephone exchanges to make connections when numbers that are dialed, would be replaced by electronic devices. His interest in his goals was very great. He stresses its importance to me so vividly that it made an indelible impression.* »

Later in the same paper when explaining the patent for the junction transistor he explains that

> « *The title « high power large area semiconductor valve » of the disclosure was not prophetic. The great contribution of the junction transistor has not been its high power handling capacity. Quite the contrary. The good high frequency performance at unprecedented low power level is what revolutionized the computer industry. This revolution has included electronic switching in telephone exhanges, <u>the objective that Mervin Kelly had emphasized to me with such enthusiasm during my first years or so at BL[13]</u>.* ».

This, this should be noted, draw the outline of an original model of research management in which the distinction between fundamental et applied research vanishes. His 1956 Nobel lecture is very interesting on this question since it starts with an appeal to go beyond the basic / applied dichotomy (on this question see Naryanamurti & Odumosu, 2016).

> « *The objective of producing useful devices has strongly influenced the choice of the research projects with which I have been associated. It is frequently said that having a more-or-less specific practical goal in mind will degrade the quality of research. I do not believe that this is necessarily the case and to make my point in this lecture I have chosen my examples of the new physics of semiconductors from research projects which were very definitely motivated by practical onsiderations.* » (W. Shockley, Nobel Lecture, 1956).

6. <u>Discussion and Conlusion</u>

This journey through Bell Labs and the invention of the transistor leads us to question the prevalent discourse of corporate research. In particular it refute the conventional wisdom, so frequent in the managerial press, that corporate research was an kind of ivory tower in the postwar period. Quite the contrary actually. We can even wonder if the Roussel & al's, « strategy of hope » really existed in the 50-60's ? In so doing we concur with K. Lipartito when he affirm that :

> "*The current skepticism about basic research in industry imagines a world of detached corportate laboratory that existed only for a moment in a much longer history of*

[13] Our emphasis.

> *successful balancing of the commitment to the long term with the need for a commercial payoff. This historical misperception perpetuates the error that basic research is a luxury firms cannot afford. Reducing the scope of innovation in this way is a recipe for reducing innovation, period.*" (Lipartito, 2009, p. 153-154)

What we have here, on the contrary, is a model of strategic research management in which the central laboratory is carefully integrated in the firms innovation process, from strategy formulation to product launch. In this perspective, the reading of J.A. Morton's *Organizing for innovation* published in 1971, is fascinating. J. Morton was responsible for the fundamental development group in charge of developing the transistors for commercial applications, before becoming VP for electronics technology at Bell Labs. He spends the entire book, based on his Bell Labs experience, demonstrating the relevance of a system approach to innovation and analyzing how relationship between BL and ATT were carefully organized from research to sales and services.

This is a significant reminder since, as pointed out by Graham (2017), the organization of research remains an important question. Its decline over the last decades may only be a a period in history. Important firms (Apple, Google…) are still investing in corporate research. In this perspective this work brings us back to the future by debunking the myth of isolated research. It underlines that internal research may remains a powerful way to overcome the challenges of risk, integration and learning typical of science-based businesses (Pisano, 2010). In this perspective, it outlines a model of research management[14] :

- anchored in the firm's strategic vision, which help to define innovative design spaces to be explored, even broadly such as « solid state physics to understand SC behaviors for telecom applications »
- inserted in the overall innovation process,
- based on multidisciplinary, mission-oriented teamwork
- and, last but not least, not enslaved to development but capable of being, at the same time, focused on pratical application ***and*** on generic theoretical knowledge (Shockley, 1950).

This last point deserves an explanation. It is interesting to note that BL make three different moves in december 1947

1. setting-up of a « fundamental development » group to start the transition to a commercial product ;
2. continuation of the research on the point-contact transistor under the leadership of Bardeen & Brattain ;
3. W. Shockley's (at first secret) work on the junction transistor.

[14] Bell Labs are not the only case of this management of research. See Le Masson & al. (2010) for analysis of the famous Nylon / DuPont case.

This means, and this constitutes a very interesting way to manage research, that they were able to simultaneously develop commercial product, explore other alternatives for SC and increase of the K base on the generic properties of SC. Indeed, as a result of his work, Shockley publishes *Electron and holes in semiconductors with application to transistor electronics* in 1950, which constitute the first codified knowledge base for the nascent semiconductor industry.

Therefore what we have here echoes the recent work on the management research, in particular Le Masson & Weil (2016) on « conceptive » vs. problem-oriented research. In the first case the first role of research is to speed-up product development, to solve the problem raised by engineering. On the contrary « conceptive » research emphasizes that a major goal of research is to explore unknown spaces (e.g. solid-state physics for SC) and to do this by designing generic laws and properties of, in our case, a class of material in order to increase their relevance for a wide range of applications. Note, as explained by Shockley and demonstrated by the transistor case, that this is not contradictory with the simultaneous development of pratical applications and the integration of research in the innovation process.

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