

## Good or not so good papers I have read or have not read.

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However simple the question raised by the Revista Matemática Iberoamericana may look, I have met some trouble to find an adequate answer. I was asked to write down a couple of pages on “the particular publications that got my attention and affected my own personal research”. The aim was to “place in a proper light the role that research journals play in the development of Mathematics”.

There are many papers that have had a decisive role in my research. However, when I look more closely at the question, there are very few ones which play that role as publications in scientific journals *per se*. First of all, the influence they have had on my research is due to the scientific content : results and methods, and not the printed medium. The most influential ones were so influential that I did not have to read them : I learned their content through books, conferences, seminars, lectures, or derived papers, and thus I actually did not have to read them, I only had to quote them in the references at the end of my own papers (thus propagating their influence a bit further) ...

Another problem was the fact that I most usually did not read them as journal papers. I remember being thrilled as a student as I read an old paper of Lebesgue in his *Oeuvres complètes*, but to know in which journal it was originally released is no longer meaningful. For modern papers, this question is even more meaningless : the most striking ones are widely disseminated as soon as they are posted on the Internet, on *arXiv* for instance, and when they are released on a specific copyrighted journal it is important to know which journal for bibliographic or bibliometric reasons, but the scientific impulse often has been exerted before the official publication. Even when the scientific publisher is important in providing the paper, most often, it is mediated through a data base of hundreds of journals, (such as *Science Direct* or *Springerlink*) more than through the production of a specific journal.

Even in the case of direct confrontation with the results (precluding any use of the huge electronic data bases offered on the Internet, or a second-hand presentation of the paper), most of the time, the actual journal is not involved, as the direct confrontation is with the author, not with the journal : either the author presents his results at a seminar or a conference, or he sends me his preprint, or his work is relayed to me by an enthusiastic co-worker. In all cases, the journal does not seem to play a very important role.

For instance, the celebrated  $T(1)$  theorem of David and Journé had a long (everlasting) impact on my research; first of all, the first draft of my Ph. D. in 1983 thrown in the dustbin, then every paper of mine, or almost every one, thereafter used this theorem. However, I never read the paper which was published in 1984 in the *Annals of Mathematics*, since I had the wonderful opportunity to meet Yves Meyer, Guy David and Jean-Lin Journé on a regular basis throughout the years 1982 and 1983.

Another of my favorite papers which I did not read was the block spin construction of ondelettes by Guy Battle, a paper which was published in 1987 in *Communication in Mathematical Physics*. I was drastically concerned with this paper, as Guy Battle constructed his spline wavelet basis in the heart of Texas in just the same week as I constructed my own basis on the sun-bathed shore of Sidi Bou Saïd. Yves Meyer received my Tunisian letter a short time after he received Battle's Texan letter; Battle was kind enough to retitle his paper as “Lemarié's functions”, as I was a younger student in a precarious status. The Battle-Lemarié wavelet basis was born. I tried to read Battle's paper, but it sounded so mysterious to me (and to every co-worker of mine) that I cannot say I really read it. It was only three years later that I understood Battle's paper, not by reading it once more, but when listening to a talk given by a young student at the École Polytechnique, whose end of year's work was to expose Battle's paper : the poor guy had great difficulties in explaining the paper, the audience in the room was totally lost in perplexity, while I had the satisfaction to feel that a three year long ripening of understanding eventually allowed me to understand a physicist's point of view. From time to time, I now find the answer to some open questions I have trouble with just by trying to think “as Battle would do”... But again, human mediation was stronger than printed rough material.

Definitely, the top paper I have not read is Hedberg's paper which, in only six pages published in 1972 in the *Proceedings of the American Mathematical Society*, established a pointwise inequality for Riesz potentials, allowing a direct and simple proof of Sobolev embeddings. I find it a seminal paper; I can track its influence in many important works in functional analysis and partial differential equations. When I heard of this paper (I was very lucky : randomly turning the pages of Adams and Hedberg's book on potential theory at my University's library, I had the surprise to see the book open just by itself to the very page where this inequality is recalled), I entered a

new dimension : now, there are distinctly two categories of analysts in my opinion, those who know this inequality and those who don't. In a recent paper of Adams about his "love affair" with Sobolev inequalities, I could find another warm tribute to this short paper of Hedberg. But, one more time, the actual journal did not play a special role, as even Adams heard very late about Hedberg's paper.

Thus far, I was at a total loss about which paper and which journal I could cite. Journals do play a central role in the dissemination of science and knowledge, peer review play a crucial role in the validation of results. But which paper should I cite?

I finally chose two papers, which had a prominent role in my research and which I encountered truly as papers published in a material journal.

The first one is a short paper by Yves Meyer : "Remarques sur un théorème de J.M. Bony", *Supplemento ai Rendiconti del Circolo Matematico di Palermo*, Serie II 1 (1981), pp. 1–20. This paper was the basis for the  $T(1)$  theorem of David and Journé, introducing the paradifferential calculus of J. M. Bony in the formalism of Littlewood–Paley decomposition. This formalism has been a faithful companion to my own work for thirty years now. But most of all, I was deeply attached to the reprint Meyer gave me, a nice well-printed and perfectly bound bunch of pages that travelled with me for years and that I read from time to time with an ever renewed pleasure : it was the first reprint I had ever had, and I loved the object. Nowadays, most "reprints" are electronic files, or hastily printed and stapled pages, and this material pleasure of holding such a little treasure is vanishing. One of my greatest pleasures in publishing in the *Revista Matemática Iberoamericana* has always been the neatly designed reprints they send to the authors. When you do some hard work, you definitely appreciate to see it enclosed in a nice environment.

The second one is a paper published some six years ago in the *Revista Matemática Iberoamericana* : Wang, W.-K. and Xu, C.-J., "The Cauchy problem for viscous shallow water equations", *Rev. Mat. Iberoamericana* 21 (2005), no. 1, 1–24. It is not "the most important" in the field, but as a matter of fact we are not asked to quote the most important papers in our field, but those which are special to us. So, why is that paper so special to me? As a matter of fact, when I read the paper, I did not feel it as very striking : the methods were not very new (they follow the formalism introduced by J.Y. Chemin for studying the equations in fluid mechanics with help of the Littlewood–Paley decomposition) and the results were far from optimal (the optimal result was given in 2008 by Chen, Miao and Zhang in the *SIAM Journal of Mathematical Analysis*). Thus, it could seem strange that I retain this paper as my special paper. But, indeed, it is special paper for me, published in a special journal. I work in a young University (it was founded only twenty years ago), in a small mathematics department. Our library is very modest. Now, we may access to many papers online, but at the beginning we had only few papers that our library received : in analysis, there was only *Annales de l'Institut Fourier*, *Potential analysis*, *SIAM Journal of Mathematical Analysis* ... and *Revista Matemática Iberoamericana* .

With such a restricted choice, I had time to have a random look to the new arrivals in the library. Thus, I found that paper on shallow water, and I read it. It was a mixture of Navier–Stokes equations (a theme I had been working on for ten years) and Euler equations (something which remained mysterious to me) Then, I read it with growing interest and dissatisfaction : clearly, the method was a method that I could understand and the result was not optimal. I could try and get a better result. But I had no time and when I tried to find some time, the optimal result had already been published. However, it was the first time I was confident in my ability to handle transport equations, and thereafter I developed my own approach of Euler equations, as a remote answer to my first dissatisfaction. By now, Euler equations have turned into a thrilling field of investigation for me.

I don't think that the "googleized" situation we all know nowadays allows such bifurcations. Finding some unexpected sources of inspiration (what is coined as "serendipity" in the science of information, following a term introduced by H. Walpole at the end of the 18th century) is not only a matter of key words and data bases. The actual meeting with the material support of ideas, such as books and journals, remains important for opening new horizons, and I still hope that libraries will not wholly migrate into the digital world.