# Is independent clinical research being supporting in Spain?

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## 1. INTRODUCTION

The present paper should not be read as a deep analysis on the state of clinical research in our country, a study that would require more time and capacity than are available to me. I modestly aim only to put down on paper a series of reflections on an activity about which I believe to have certain authority and experience: the independent clinical research that has and is being conducted in Spanish hospitals and the blindness or incapacity to support it. After many years of complementing my patient care activities with clinical studies, I have built a considerable research curriculum, but which holds a record of dubious honor: the virtual total absence of government grants. I feel obliged to make the following reflections because I am absolutely certain they are shared by many other colleagues (both nephrologists and nonnephrologists) and also because there is a notable lack of written discussion on this crucial issue. I will not only give vent to my hardships and complaints, but also propose some possible specific aids, with the somewhat remote hope that they will be heard by our scientific and health authorities. And, in short, if this editorial serves to create some controversy, it will be more than welcome in the somewhat quiet current panorama of Spanish nephrology.

## 2. DELINEATING THE FIELD; WHAT DO I MEAN BY «INDEPENDENT CLINICAL RESEARCH»

To focus the topic clearly, I will clarify what I mean by the term «independent clinical research», i.e., what types of research I do not include in this editorial:

**2.1.** Logically, I exclude all basic research of any type whatever that is done (and I believe this is a progressively growing category) in our hospitals and in many other institutions connected or not with them. I also exclude those studies (even if focused on patients) that require the provision of more or less complex or costly technical equipment or facilities (laboratories, research animals, specific techniques, etc.). Although I must advance with caution in this field because of my limited experience, my impression is that particularly since the creation and development of the Health Research Fund (FIS) we have witnessed a larger and sustained institutional support for this type of studies. The problem, as I have sensed in some of my talks with health officials, is that, like the public lay in the subject, they are reluctant to conceive as research (and therefore to support it with specific measures) something that does not used test tubes, microscopes…, in other words, that does not match the typical cliché image of a scientist doing research in his laboratory. How can we call a clinician who spends his afternoons reviewing the medical histories of his patients a researcher?

**2.2.** I also exclude, by definition, all clinical research sponsored and directed by pharmaceutical laboratories. It is obvious that most prospective randomized controlled studies published in leading medical journals and which are (and this is a point all too often forgotten) the most valuable tool analyzed by evidence-based medicine (EBM) are funded and in most cases sponsored by the industry. I do not wish by this to question or raise doubts about the scientific excellence and seriousness of these studies. However, as is only logical, these laboratories mainly sponsor those short or long-term studies that are profitable for them, and there is nothing criticizable about this. The investment required to conduct one of these multicenter or multicontinental studies that we are accustomed to reading in the New England Journal of Medicine or similar journals, which prove or disprove the efficacy of a certain drug or therapeutic intervention, is enormous, as also is (and increasingly more so) the administrative and organizational complexity required by such studies. This means that most laboratories are very reluctant to support studies which are not developed in their setting or which have objectives not related to their specific

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interests. As could be expected in this panorama, we see impeccably designed studies with highly interesting results, but which are almost always focused on novel (and generally very expensive) drugs. And, I repeat, the pharmaceutical industry should not be blamed for this; it has its own logical interests and objectives, with imperative demands for economic profitability. We should even reflect on and discuss more deeply how abandonment by public institutions of fields such as continuing education, medical congresses, research… has made us dependent to an extreme degree on the aid of the industry.

## 3. NEED FOR INDEPENDENT CLINICAL RESEARCH

I should not spend many lines on something whose importance is obvious. In summary, this research implies a continuous reflection and analysis about the essence itself of our profession as clinicians. In its retrospective dimension, it provides an approach that allows the description and characterization of unrecorded or little known clinical entities, associations or events. Progression, prognostic markers and therapeutic interventions that can alter the course of a particular disease can only be described with long-term follow-up of cohorts and intelligent and conscientious analysis of these cohorts. After years of somewhat irrational veneration of the prospective controlled trial (as said earlier almost always sponsored by the pharmaceutical industry) as the basic pillar of EBM, numerous authorized voices have been pointing out in recent years the crucial value of these retrospective clinical studies.[1] After all, the analysis of these cohorts requires us to review the strictest reality of our patients, without the artificiality always imposed by prospective clinical studies: exclusion of many patients with the same disease who don't «fit» the right profile (because they are too old, too seriously ill, not obedient enough…), performance of laboratory tests at intervals not scheduled in routine clinical practice, etc. This separation from «ordinary life» explains to a large extent the apparent paradox of diametrically opposed results being obtained in prospective studies of similar design, or that beneficial results of a certain measure in a prospective trial are not reproducible in everyday practice We experienced a paradigmatic example of such a danger with the RALES study,[2] which showed the beneficial effect of spironolactone in patients with congestive heart failure, in an impeccable design that included rigorous controls of renal function and serum potassium. But some years later, a study, this time retrospective,[3] showed how the incidence of deaths attributable to hyperkalemia had rose dramatically after publication of the RALES study….and there are many other examples we could point out.

The foregoing should not be interpreted, of course, as an absurd disqualification of prospective controlled trials, which are today the best scientific tool available to analyze and evaluate many questions from our activity as clinicians, particularly all those concerning the efficacy and safety of therapeutic measures. But retrospective cohort studies, whose value for many years was underestimated, are a totally necessary counterweight to put the results of prospective trials in their true dimension, in addition to covering all areas of clinical research (analysis of prognostic markers, clinical associations, characterization of entities…) that cannot be investigated using prospective trials.

But in the tumultuous world of prospective clinical trials there is also an imperative need for the entry of research sponsored and directed by clinicians with total independence from the industry. Clinical researchers should have effective support (we will discuss this later) so that they can answer their questions about the value of many medicines whose low cost makes them unattractive for the industry. One of my criticisms of unscientific and uncritical use of EBM is that it is unable to realize the enormous deviation introduced by selective funding of certain trials by the industry; for example, we have numerous prospective trials with angiotensin receptor antagonists, but who is going to investigate the value of steroids in idiopathic focal segmental glomerulosclerosis or the potent antiproteinuric effect of the modest and outrageously cheap spironolactone?... And, on top of this, we still have to read and hear over and over that «…there is no quality scientific evidence available on the value of steroids in nephrotic syndrome, etc., etc., etc.». And what can we expect with the current state of affairs? For all the above, it is essential that we maintain a spirit of critical and independent analysis of scientific activity,[4] as is being done in an exemplary fashion in our specialty with the supplements on «Evidence-Based Nephrology».

## 4. IS INDEPENDENT CLINICAL RESEARCH BEING DONE IN OUR COUNTRY?

Of course it is being done, and within the existing limitations, it is of very high standard. It never ceases to amaze me how year after year, in the total absence of curricular or economic incentives, many nephrologists (because this editorial is addressed to our specialty, but it would be applicable to all) continue to contribute to publications and congresses with their series of cases, their clinical observations… It is my opinion that this «free» activity (we are paid for seeing patients, not for doing this), as well as the increasingly infrequent hospital clinical sessions, are one of the most beautiful and stimulating exercises of intellectual resistance against those authentic

cancers called clinical management, management by processes or other similar nonsense, which, after having completely ruined hospital medical managements, now threaten to directly invade medical departments. But this would be a topic for another occasion…

There are many examples of national clinical research I could turn to, but I will focus on tow specific studies because I participated actively in them and because they also offer other characteristics of interest: their multicenter nature and that one is an example of a retrospective study and the other of a prospective study. I will summarize the internal development and difficulties encountered in both studies so that they can serve as an example at the end of this editorial for specific proposals.

### 4.1. A retrospective study: influence of steroid treatment on immunoallergic interstitial nephritis

A study in 13 Madrid hospitals has recently been published in Kidney International which included a large series of patients with immunoallergic interstitial nephritis[5] and analyzed the beneficial effect of early steroid treatment in these patients. The study has had considerable impact[6] and will probably become a reference study on this topic for many years. But what I would like to highlight from this study, in view of the objective of this editorial, are the following aspects: 1) the idea for the study arose, as occurs almost always in this type of research, from isolated clinical observations and review of a small number of cases which served as a clue and a stimulus for a larger scale analysis 2) the protocol used included a simple but precise table for collecting data preceded by brief indications on the type of patients to be included and excluded. A total of 5 pages long, a far cry from the grueling protocols required to request grants from the FIS or other public institutions. 3) The study was started and ended without any grant. 4) As in all these studies, material expenses were negligible (ball point pens in the first phase and computers in the second). However, the effort made by the researchers was intense, generous and sustained, as is well known by all who have fought to publish their papers in high impact peer-reviewed journals (and I do not include here the commonly used false route of unreviewed supplements paid by the industry). This effort includes: collection of cases (many department still lack a diagnostic coding system to allow rapid identification of patients), review of the medical histories for scientific purposes (which is in itself a fight in many hospitals), transcription of the handwritten data to a computerized database, the statistical study, interpretation and discussion of results, writing of the manuscript (in English) and its discussion. And finally submission to the journal and the fight with the reviewers In short, hours and hours of huge efforts, never taken into account except when using these studies to boast about the hospital in question (but often without citing the authors).

### 4.2. A prospective study: Tacrolimus in membranous glomerulonephritis[7]

As in the previous case, the idea arose from isolated clinical observations with the use of calcineurin inhibitors in membranous nephropathy, which led a group of nephrologists to propose a prospective randomized study to compare tacrolimus monotherapy versus conservative treatment of this condition. A complete protocol was written for the study (rationale, inclusion and exclusion criteria, treatment of both groups, randomization system, assessment of number needed to treat, visit protocol...), all that is usually required in this type of protocols. This is an important point that I want to stress: while for a prospective study of this nature, which should be evaluated and approved by the funding organizations and ethics committees, it is absolutely necessary to prepare such protocols, it is not or should not be so for retrospective studies such as the one indicated in point 4.1. In these studies, such protocols are, if imposed, a waste of time, owing to the different dynamics of the two types of studies.

Once the protocol was completed, it was submitted, not to any public institution, but for consideration by the pharmaceutical laboratory that owned the drug to be studied. On this occasion (although in the author's experience this is very pleasing but rare fortune), the laboratory took charge of funding of the study through a contract research organization (CRO). And here we find a key element in this type of prospective studies: CROs are private companies specialized in the startup and conduct of these studies. Since many years ago they have become indispensable elements in research because of to the enormous complexity involved in these studies, a task beyond the capacity of ordinary hospital physicians: management of the protocol through the clinical trial committees (if it is a multicenter study, such as the one that concerns us here, the complexity skyrockets), management of the insurance required for patients…. If a group of physicians designs a prospective therapeutic study and receives funding for it from a public entity, it will be indispensable divert these funds to a CRO, to implement the countless administrative steps we pointed out above.

In the case that concerns us here, these steps were positive and relatively rapid, and all participating departments were provided with the usual materials in this type of studies (case report forms —electronic in the more modern ones—, consent forms, and all bureaucratic steps resolved). But apart

from this, the effort of the researchers was the same or even larger than that described in 4.1.: collection of data from the visits, transfer to the database, the statistical study, writing and discussion of the manuscript, the hard work of publication… in addition to directing the patients included in the protocol over the many months that the study lasted. I should point out here a characteristic of this study which in my opinion should be shared by all studies: in addition to the initiative for the study, the nephrologists promoting the study carried out the statistical study, writing, discussion and process of publication of the manuscript, while the laboratory, in an exemplary manner, had the sole function of funding the study without participation in evaluation of results.

## 5. THE INSTITUTE OF HEALTH CARLOS III (ISCIII), HEALTH RESEARCH FUND (FIS) AND INDEPENDENT CLINICAL RESEARCH.

As this editorial aims to be politically incorrect, I will be frank in my opinion as a primarily clinical researcher about the ISCIII and FIS. I said before that I believe that the FIS has been very positive for biomedical research in our hospitals and I assume there are numerous biometric studies to support this assumption. However, the focus of its activity has been on basic research or, in a broader sense, on all studies that require evident economic support, either because they use experimental animals, reagents, laboratory supplies, special techniques or whatever other reasons. With this, it has put aside support for purely clinical research, or at least it has erroneously sought to support it by applying criteria and approaches transferred from basic research. If we consider the retrospective clinical research exemplified in point 4.1., who will dare to request support from the FIS for studies who material expenses are nil? Unless one were to evaluate (and it is not a joke) economically the time and effort employed by the researchers and request compensation for them. But the dynamics of this type of retrospective studies, agile and without material requirements or the need for laborious protocols, has led many authors(and I include myself among them) to do without the tedious bureaucracy and slowness of official grants from the FIS. Moreover, the general impression among clinical researchers is that simply clinical studies are at a disadvantage when it come to the FIS evaluation, and the simpler and «cheaper» the study is, the fewer chances it has of receiving support. It should be pointed out here, against a certain «disdainful or superior» attitude of basic research, that the main differences between in studies are in their originality, significance, rigor and the standing of the journal where they are finally disseminated, and not in whether they are «basic» or «clinical». Unfortunately, it is not exceptional for basic or mixed (clinical/basic) research groups to accumulate grants based on research projects (see the section below on «Glorification of the project») that hardly generate any publications or only in journals of very low standing.

I will be told, and quite rightly, that it is very difficult for the FIS or other similar public institutions to evaluate this type of retrospective clinical research. To do so would have required an effort of imagination that has not been made to search for novel mechanisms of support. But, what is the situation with prospective clinical research, the randomized study that is nowadays totally dependent on the pharmaceutical industry? It has also not had any type of official support, as proven by the fact that only very recently has a specific figure been created to support this type of studies: the subprogram of Noncommercial clinical research projects with medicinal products for human use. It is a very interesting initiative that could start to palliate the lack of institutional support we have been commented on. But, and here we already start to see the traditional problems reproduced, as bureaucracy strike again: there is only one annual call to apply for grants and furthermore only a very short period between publication of the grant and the end of the application period! My opinion is that if the ISCIII or the FIS seriously wish to become an alterative to the industry in supporting these studies, they should reproduce the model and speediness of grant aid that clinicians obtain (though rarely) from the industry, and which was illustrated in the study discussed in point 4.2.

## 6. GLORIFICATION OF THE PROJECT. FROM PROJECT TO PROJECT AS AN END IN ITSELF.

In any research community, it is accepted that publication in a scientific journal (and the greater its diffusion and impact factor the better) is the culmination and ultimate goal of any study, whether it is basic, clinical or any other type. Presentation of the results at congresses in another type of complementary and highly commendable diffusion, but it should not substitute for publication. Any author of scientific publications in demanding journals known the immense effort required and this perspective must be taught to young researchers. Perseverance and the ability to overcome setbacks (how many of our papers are published in recognized journals only after being previously rejected by two or three others!) is essential if one wants to do something in this field.

The foregoing is intended to serve as an introduction to a peculiar phenomenon that has appeared in recent years, but which is experiencing a worrying growth: the assessment of

the project for a study submitted to the FIS or other public and private institutions, irrespective of whether it has been published or not. In most current curricular evaluations, a large margin is granted in the assessment for participation in these projects and I have seen in some university scales for research merits that there is only a space for these projects, with no reference to publications or even congresses. It is understandable that the assessment should given some value to these items, but certainly a research project that has received a juicy grant and has nonetheless failed to generate any relevant scientific publication should not be presented as a merit by the researcher(s)…but rather be penalized for squandering public funds. Jokes aside, this dangerous trend can lead to the absurdity of groups that self-perpetuate their funding from years spent «in the fold», accumulating pseudomerits based on projects granted aid but with hardly any publications. And, on the contrary, research groups with a «real» production of publications but which have worked outside of «the fold», without subsidized official projects, are absurdly penalized. As I said at the beginning, this latter group includes many groups that have done independent clinical research in our country, and so it is urgent to put a halt to this outrage. Moreover, as I also said earlier (see section 4.1), retrospective clinical research is intrinsically independent of this type of previous projects, and consequently should only be assessed on the basis of actual scientific publications. In summary and to be straight to the point: we should assess actual research (publications) and not projects, no matter how grandiose they may sound.

## 7. MULTICENTER AND TRANSLATIONAL RESEARCH

The multicenter nature of a research study enhances its possibilities, and this statement is particularly applicable to nephrology. There are diseases in our specialty whose low prevalence requires the contributions of various departments: a good example of this is glomerular diseases. Since some years ago, I have had the fortune to participate in or direct multicenter studies, such as those used to illustrate points 4.1 and 4.2. In addition, since little over a year ago, I have had first-hand experience in the creation of a SEN working group (GLOSEN), whose basic objective is multicenter study of glomerular diseases. Based on all these experiences I can state that the willingness of many nephrologists to collaborate is really striking. In view of the homogeneity and generally high professional standard of nephrology departments in our country, the potential of these multicenter groups is enormous and they should be privileged recipients of grants for independent clinical research, both retrospective (for example, GLOSEN is collecting retrospective cohort studies not comparable to the studies published to date) and prospective (basically prospective therapeutic studies). But another important conclusion is the physicians participate enthusiastically in these studies when the ideas are attractive, there are little or no bureaucratic obstacles, there is an atmosphere of openness and cooperation and when it is clear that the research is being done for it own sake, i.e., for the sake of our profession and our patients, which is deep down what motivates us or should motivate all of us.

While up to now I have only spoken of pure clinical research, a consequence which one might say is inevitable, but which is also totally desirable, is that publications on merely clinical facts and data may provide clues for more basic research in many different directions (genetic or molecular studies, animal models, etc.). The author has had very interesting experiences in this regard, which I would have liked to describe if I had more space, since they illustrate how from the observation and description of a small number of cases (8) studies or collaborations of international scope may arise in many other fields (9). In other words, an excellent way to promote basic or translational research is to support also clinical research studies, no matter how «purely clinical» they are. They will grow and branch out if the ideas are good.

## 8. HOW ARE CLINICIANS ABLE TO DO RESEARCH?

A statement repeated like a mantra by any manager of whatever hospital is that «We must do research, research is fundamental…». I think that the right question should be: how are clinicians able to do research in the current hospital panorama? As I said before, my experience tells me that physicians do research above all for a love of truth in their profession. Because in reality, no specific stimuli for independent clinical research exist, as we shall see.

### 8.1. Does one earn money doing research?

Of course one can, but not doing independent clinical research. One earns money by participating in research sponsored by the industry, and in fact, I believe it is one of the leading motivations for participating in numerous research «protocols» sponsored by the industry. I previously mentioned the pros and cons that I see in research sponsored and organized by the pharmaceutical industry, but I can add that in most of these protocols the participating physician works in a passive role and only applies the criteria generated in other settings. This research should therefore not be confused the praiseworthy and long-suffering independent research I have been defending. Nevertheless, participation in these protocols has several advantages: it provides and essential influx of money

to support hospital research foundations and, in a broader sense, a recognition of the capacity and prestige of the department requested to participate in them.

But, getting back to our topic, why can't one earn money doing independent research? For example, quantifying the publications (not projects, please) generated annually by a department or a specific person with modern bibliometric criteria (impact factor, number of citations, «h-index») and assigning an amount of money to them to be paid according to the variable productivity. I can already see so-called experts throwing up their hands in horror, but take note: there are more and more persons in hospitals who are paid to do research or to help in research (staff from research units, foundations…). And, nevertheless (and this a matter that is kept carefully concealed in most hospitals), a very high proportion of hospitals' scientific production is generated by clinical physicians, whose remuneration comes from seeing patients.

### 8.2. Are we given time, personnel or material means to do research?

Absolutely not. For a clinical physician, independent research means (and I can vouch for it from first-hand experience) evening after evening and whole weekends devoted to it. And the solution is not to separate this researcher for his or her clinical activity. Independent clinical research is closely linked to clinical activity, since from it come the experiences, ideas or «sparks» of clinical observation…For this reason, systems such as those currently implemented in some hospitals to hire trainees or physicians on temporary contracts (6 months, 1 year…) to help in clinical research have little future: once again, it is an idea copied from basic research (laboratory trainee) and furthermore at a time when there is an alarming scarcity of physicians, nobody (this is already occurring) is going to accept this type of subcontracting. Therefore, the solution is to recognize the scientific activity of a specific department (publications, I insist, it is quite easy if one wants to) and to determine staff size according to it. For instance, departments with sustained high quality productions would receive one, two, or three (depending on quantifiable parameters that could be consensuated fairly easily) indefinite temporary contracts for physicians who would act as a member of the staff for all purposes, both clinical and research related. Of course, these staff expansions would be reviewable, if this department stops publishing, it would lose these contracts.

The staff expansion, justified by the research activity, would partially free the physician who generate this research and give them more time to devote to this activity. But not only with personnel and time can we be helped. As described in section 4.1. on retrospective research, the availability of medical history records to aid the researcher, the creation of diagnostic coding systems for departments (an absolutely crucial aspect to ensure adequate review of a specific disease; many departments work «by memory»), the hiring of clinical statisticians, documentalists, secretarial staff focused on providing assistance to the clinical researchers would be tremendously valuable aids.

### 8.3. Can one go up the promotion ladder for doing research?

Not at all. Some time ago mediocrity won the battle in all hospital and university curricular evaluations. In the section on research, there is always an assessment by points, but with an «upper limit» that carefully preserves mediocrity in our setting. Try it yourself, with a reasonable number of, say, 10 or 15 national publications (we don't want to overdo it), one easily reaches the upper limit. This means that it makes no difference if one has 10-20 national publications or 100 publications in the New England Journal, Lancet, JASN or whatever else, but, of course, while scrupulously respecting legality.

And the same thing happens with our professional career, which as could be expected has been transformed into a system to award seniority, in which care quality (how do we evaluate this is another pending subject) or scientific activity (here we will also surely find the famous «upper limit») are lacking any real stimulus.

## 9. THE ROLE OF HOSPITAL RESEARCH FOUNDATIONS

Over the last years, many hospitals have begun to have institutions that are ideal in theory to solve or at least palliate the problems and obstacles that I have tried to summarize up to now: hospital research foundations. While I have spoken of the shortcomings of the ISCIII y FIS concerning independent clinical research, I think or I wish that in coming years these requests will be solved closer at hand within the hospitals themselves, but based on criteria of common sense and stimulation of real hospital research. And, of course, assuming that the above mentioned shortcoming will not be reproduced.

As is logical, all basic and translational hospital research should also be reinforced with these foundations. But let us hope they will be the definitive tool that will recognize and foster independent clinical research in all of the aspects

mentioned. Considering that one of their major sources of funding is the percentage they retain on the payments made by the pharmaceutical industry for their research projects and that these are attracted and conducted by clinical physicians, it should both be expected and required that these funds serve to support independent clinical research.

However, first we need to define clearly the field and clarify what type of research is done in each hospital, its quality and who it is done by. Oddly, we are seeing that many hospitals carry out «global» bibliometric analyses to describe the scientific production of the hospital in general terms, or at most by areas of knowledge, but while carefully preserving the anonymity of the individuals whose effort, ideas and initiative have made these achievements possible. Of course, one of the key objectives of these foundations is that research activities are carried out by the largest possible number of departments and units, but without reducing the merits of those that already do so and falling into the typical error of «bottom up standardization». Let's see if some high-ranking hospital research professional (this should never be an excuse to «escape» from dreary clinical assistance) is unmasked…A periodic publication in each hospital of the bibliometric rankings by departments and individual researchers would be highly stimulating.

Foundations should also clarify the functions and objectives of currently existing research units: I mentioned previously the decisive importance of providing support units to clinical researchers (statisticians, documentalists…). But the objective should be this: to support and not to compete with other departments for resources, research funds, etc., as occurs in some hospitals. This misunderstanding should also be resolved by these foundations, which are expected to have a clarity of ideas and openness that will make hospital research of whatever type an easy and amenable as well as exciting task.

## 10. SOME SPECIFIC PROPOSALS TO CONCLUDE.

And to conclude, I summarize some specific proposal that I have been describing more by the order in which they appear the text than by their importance.

– Facilitate medical history review for retrospective studies: documentalists, secretarial staff, ad hoc organization of medical history records.

– Facilitate and support the creation of intelligent and scientific (not administrative) diagnostic coding systems at the hospital level or (better still) by departments and units.

– Availability of clinical statisticians, epidemiologists, translators… «freed» to support clinical researchers, not of the kind «I have my own research work, I have no time» or «I'll help you if you include me in the paper».

– For prospective controlled therapeutic trials, perfect the «window» open by the program for noncommercial clinical research projects with medicinal products for human use, by means of the following suggestions:

a) Abandonment of 19th century slowness and bureaucracy with a continuously open «window» for trial grant requests or at least quarterly calls for grants.
b) Reproduction by public institutions of the ease and speed provided (when grant is successful) by the pharmaceutical industry for these trials. Automatic hiring of a CRO (why not create a public CRO?) to release researchers from the numerous and increasingly exhausting bureaucratic tasks.

– Priority support for multicenter research of whatever type, with support and funding of its specific needs.

– Recognition of independent clinical research by hospital foundations, identifying the departments that are conducting it and meeting their specific demands. And making sure that this new mentality is made known to the many department that have given it up.

– Recognize real research production through its final result: publications, assessing quantitatively and qualitatively (impact factor, citations, «h-index») the scientific production of each department and each researcher.

– Abandon current overvaluing of the subsidized project as a merit in itself, regardless of the publications it has generated.

– Assess the possibility of economic compensation (through variable productivity or other systems) of scientific production.

– Recognition of the scientific production of each department, promoting staff physician positions with reviewable indefinite temporary contracts according to consensuated parameters to assess this production.

– Eliminate current «upper limits» that prevent a true assessment of research activity in hospital and university curricular evaluation scales.

Of course, there are many other ways to stimulate and provide incentives for the objective of this editorial, independent clinical research, and there are many topics and nuances that I have had to leave out. But if any of these ideas, or rather the general need for support that I have wished to reflect in this text «takes hold» in scientific and health authorities, I will be more than satisfied.

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