

|Leadership and innovation. How consensus management blocks genuine innovation.

Editorial published in Bioscience Hypotheses. 2(5) (2009): 277 - 281

William Bains

ABSTRACT

The consensual basis for selecting research topics works for incremental innovation, but is the enemy of radical breakthroughs. Consensual processes, from the 'European Year of Creativity and Innovation' to peer review, should be supplemented by the leadership of those with the courage to back the extremes. Such leadership requires support from the very top of scientific management and funding, in academia and industry.

TEXT

About the time this editorial is published, the European Year of Creativity and Innovation will be concluding with a Gala Conference in Maastricht in Belgium. The programme claims exciting events to continue the years' work stimulating creativity across the EU, which participants can benefit from for a modest fee.

Readers outside the European Union (EU) probably have not realised that 2009 was the European Year of Creativity and Innovation (hereafter EYCI). They are not alone. In June 2009 I polled 83 people, nearly all in the UK, all at the heart of scientific and technical innovation in one way or another. Only five had even heard of EYCI, and only one had seen any effect of the Year at all (some workshops in his region had been branded as EYCI events) (Figure 1). By any standards this must be considered less than effective. I found out about EYCI from reading a US-based trade magazine. Had I not had free time and a flat laptop battery in Atlanta airport, I too would be ignorant.

Readers within the EU might wonder what this clearly unsuccessful attempt by the supranational government to stimulate us to invent and create is costing, something the EU refuses to tell us. But for Bioscience Hypotheses the EYCI has broader implications, and for that reason is worthy of our scrutiny.

Why did anyone in the European Commission, the administrative body of the EU, think that organising the EYCI was a good idea? There is a substantial organization behind it. There are local coordinators and central initiatives which can be found after some searching, and of course conferences and workshops. This takes time, effort and money. Why do it?

It is almost axiomatic in modern economics that growth of the economy is fuelled by innovation, and specifically scientific innovation. Whether this is true is another matter, and one I have discussed elsewhere (1) as have others (2,3). It is pretty clear that most economic innovation is not related to technical innovation, and that technical innovation and creativity rarely leads to economic success, and that scientific innovation is only loosely connected to technical innovation. However

technical innovation is important, and in the long run new technology, together with a much larger population, is why our standard of living is higher today than it was 1000 years ago.

We are constantly surrounded by technical innovation. My phone was out of date before I bought it (it was a cheap phone), my software is old within months, I am told that my toothbrush, kitchen cleaner, car, running shoes, television, furniture are all outmoded by new, innovative products. But what we want is not this continuous, rather tiring, incremental innovation. We want major, game-changing innovation, things that will make our lives radically better. This is the innovation that is held out as examples by the proponents of approaches to stimulating innovation – breakthroughs of equivalent impact as the jet engine and the web browser and MRI. We want to take Giant Leaps, not small steps.

Discussion of Giant Leaps is appropriate in this 40th anniversary of Apollo 11. The US space programme in the 1960s is often cited an example of innovation at its best, a view I support. Neil Armstrong knew what he was talking about when he said that his step onto the lunar surface was a giant leap for mankind. It was not a scientific leap – we had known where the moon was since classical Greek estimates of its distance, and the physics of how to get there since Newton. But the technological risks and challenges were huge, and Apollo stepped up to them. It is a commonplace observation that none of today's government space agencies in the West have that daring, and their space programmes are the poorer for it.

I was privileged to listen to talks by some of the pioneers of the Apollo programme at a seminar at MIT earlier in 2009. Their enthusiasm and 'can do' attitude was of course helped by an essentially unlimited budget, but also by a focus on project rather than process which has been lost in today's science. When faced with a problem, their approach was, in essence, to throw a lot of engineers into a room and tell them solve it. (The film *Apollo13* illustrates the approach well, even if many scenes were simplified and shortened for dramatic effect.) And they did solve problems.

What this approach does not have is a formal process. There is no algorithm for making decisions that could be written down. There were clear responsibilities, but the responsibility was for output, not for process.

This was rare in large scale R&D programmes even then, and is now almost unknown. Instead, almost all large-scale research endeavours are driven to develop processes. 'Process' means that every decision must be arrived at by an explicit method, an algorithm, every activity must be definable beforehand in terms that can be reported. The key word was, and remains, 'transparency', but this does not mean what a scientist might mean by transparency. To a scientist, transparency means that the data is clear and available for anyone to see. To a management consultant it means that they understand the process through which an activity is conducted or by which a decision is made.

In some contexts, such as jurisprudence or taxation, it is clearly important to have process transparency, so everyone understands the rules under which we operate. But in research, to demand that the process by which innovation happens is defined before that innovation occurs, even before we have any idea what innovation is needed, is

often stifling. And it does not allow for the truly talented to flourish, as inevitably they are stepping outside what the merely competent can think of, inventing new thought processes, new lab processes, as they go along.

Mainly for this reason, scientists and engineers have come to dread the introduction of 'process' into research (see for example Figure 2). They see 'process' as a valueless imposition on their work of solving problems and creating new technology. This is a mistake, as many processes, such as basic quality and checking standards, standardization of assay techniques, validation of methods and so on are critical to doing science that is meaningful.

But to try to extend this to every aspect of the intellectual endeavour is clearly silly, as Dilbert illustrates. The result of doing so is that science becomes dull and mediocre, as has happened systematically in the West in academia where dogged persistence in doing the same thing for two decades is rewarded over innovation (4). Speaking at a conference in Boston this summer¹, Jo Bolen, until recently CSO of Millennium Pharmaceuticals, put much of the blame for the decline of productivity of the pharmaceutical industry on the imposition of theories of organizational management on its R&D, with their requirements for an explicit process for everything. The call for 'process' is a call for mediocrity.

If a focus on management process drives towards mediocrity, why do it? I believe that the principle attraction of 'process' is that no-one is to blame. A goal is set, and there is a process for setting goals, usually one involving a Scientific Advisory Board and a lot of consultants. We might, for example, set the goal of discovering candidate drugs by screening a million compounds for pharmacological activity against receptors *in vitro*. This is a good, process-defined goal. We can define sub-goals and entirely specific actions for all the parts of the process in reaching the goal. We select the target receptors, itself a process that needs a goal, a path and so on, clone and express them, optimise an assay, execute it on robots and so on. If at the end you do not discover any drugs, well, no-one is to blame because each did their part of the process. Blame is distributed across the organization: indeed, in a genuine sense the organization is to blame, because the organizational structure required that there be a process, not a decision.

By contrast we might employ one brilliant chemist and tell them to invent a new drug. It is a well-established observation that most new breakthrough drug discoveries come from maverick individuals, be they oddball company chemists, anarchic biotech start-ups or (occasionally) talented academics, and not from process-driven corporate research. However this also might also fail, and then the chemist concerned, and whoever hired them, are clearly to blame. That is a career-limiting endpoint.

So taking decisions about research direction, and funding, without an extensive, blame-dissipating process, takes courage, a believe in your own judgement, the willingness to take responsibility for being wrong as well as a rewards of being right. In short, it requires leadership.

¹ Massachusetts Life Sciences Innovation Day, June 3, 2009, Sheraton Boston Hotel, Boston, MA, USA

This is not leadership as practiced by many Western companies, universities and governmental institutions, where leadership is either confused with management or with direction. Management is the skill and talent of making sure that things happen smoothly, that resources are allocated appropriately, that people are happy and motivated to do what the organization wants. In short, it is supporting the process. Direction is telling them what the organization wants, in short supporting the establishment of the process. Both require organizations that are 'transparent' in the management sense, of having clear algorithms for making decisions that everyone agrees with, and of making sure that everyone's adherence to the process is documented. Both try to tell people how to be innovative by fitting into the organizational behavioural algorithm.

These models of leadership – management and command – are the leadership models of the hierarchy, which is why they are so loved by huge institutions such as the EU, Western universities and big pharmaceutical companies. What creativity and innovation need are the leadership of the clan, where the leader is the one out in front showing the way (5). The leader takes a stand that says '*This idea, this scientist is outstanding. Let's support that.*' Managing a consensus is not leadership. It is management, because it takes the ability to decide away from people and invests it in a process. At best this results in consensus, at worst in no decision at all (usually expressed as a decision to study the problem more with a longer, more expensive committee-based 'process'). Consensus is not the way to be innovative. As Charlton has pointed out elsewhere, genuinely new ideas are almost always derided by the consensus at the start (6), because they are outside the algorithm the consensus has defined for agreeing what it will consider. Any process that is more complex than autocratic, arbitrary decision is bound to end up consensus focussed, and hence mediocre and not innovative.

A classic example of the role of consensus non-leadership in research is Peer Review. The peer review method of valuing a new idea works very well for Small Step innovation, things that are a slightly surprising next step along a well-worked path of discovery or argument or development. But it fails rather miserably when applied to radical ideas.

As an example, Figure 3 shows the number of Medline papers and the number of news stories surrounding the topics of cancer stem cells. The first of the current wave of 'cancer stem cell' papers came out in the late 1990s (7). In fact the idea that cancer develops from a small population of stem-like cells was developed in the 1970s for some haematological malignancies (8,9), itself a follow-on from observations in the 1950s that show that new tumours develop from cells that are rare in bulk cancer (reviewed in 10), but not generalised or explored further. One could argue that lack of tools to characterise stem cells was the reason that observations from the 1970s were not taken forward (although limiting dilution methods used to demonstrate stem cells in brain (11) and breast (12) were essentially extensions of the 1950s techniques to the automation age). However by 1995 the tools and the underlying concepts were ready, once a single breakthrough experimental paper had shown the way.

So, the conventional view would say, this valuable new insight into cancer should have been picked up and explored with further scientific research and publication in the 2 – 3 years needed for the original papers to be read, replicated, and new results

generated and written up. In practice the ideas were largely ignored. Only after the general press picked up the concept in 2002 did scientific attention begin to be paid. By contrast iPS cell technology, another currently trendy bit of science emerging from a growing set of tools to explore stem cells, follows the conventional path – as soon as the technique was shown to work (13, 14), it was taken up by other scientists, used and published (Figure 4).

The delay in taking up cancer stem cells as a research topic was not caused by a lack of interest in stem cells in general during the period, as analysis of the general press and Medline shows (Figure 5). It was because cancer stem cell biology was not accepted by the process that deemed what biology was practical, fundable, useful and publishable. The standard view of cancer was one of a bulk of mutating cells, not a source and a sink. So a result, even a clear result based on a long experimental history, did not fit into the consensus and the process for deciding what research to do next rejected it as being a topic suitable for large-scale investment. How much more might we know now about cancer if this idea had been able to penetrate the high wall of peer consensus in the 1980s rather than two decades later? By contrast iPS cells fitted perfectly with then-current orthodoxy, that stem cells can be ‘programmed’ genetically, and the ‘only’ surprising aspect of the original papers was how few genes were needed to drive a stem-like phenotype.

This makes a testable prediction (as is required of papers in Bioscience Hypotheses). Bioscience Hypotheses and Medical Hypotheses should be good barometers of what new ideas scientists are thinking about. We do not require years of experimental work, and preceding years of grant applications. So the ‘cancer stem cell’ (CSC) idea should have entered the scientific consciousness sometime around 1998 (long before we were publishing), around the same time as the idea of genetically reprogramming stem cells. The ratio of CSC/iPS papers should be pretty stable over the last two years. If however the number of papers published is dependent not on scientific ingenuity and invention but on a consensus of what is interesting (i.e. the number of papers appearing in the scientific press), then the ratio CSC:iPS should fall during 2008/9. Figure 6 shows that it does, quite substantially. At the end of 2007 ‘cancer stem cells’ was the *topic du jour*. By 2009 iPS cells had overtaken them.

(I recognise that this is a crude measure, and specifically does not cover a long enough timescale to be very convincing. The reasons for this I will expound in the next issue. But it is a strong hint that fashion, not genuine insight, drives even the creative and innovative end of the scientific endeavour represented in these pages.)

The late David Horrobin realised this when he set up Medical Hypotheses, and that journal’s current editor and I also subscribe to the view that judging innovation by a fixed algorithm based on consensus is not always the best way to bring new ideas forward. Hence the philosophy of our two journals, which brings me back to leadership, and the EYCI.

Movements like the EYCI are well intentioned but ultimately futile, as by trying to please everyone they end up directing their efforts at what everyone agrees on and accepts, which by definition is not innovative. Political bodies are particularly prone to rely on a ‘process’ philosophy, and the EU is infamously prone to it. So a year of innovation looks back to the past and the established, the consensus. This is the

antithesis of innovation, and usually slightly repugnant to the truly creative individual. Not surprisingly, its impact is not great.

The clan approach to leadership is to lead from the front, to demonstrate the value of an idea by doing it. This is the philosophy of Bioscience Hypotheses and Medical Hypotheses. The journals select papers primarily by editorial choice (15, 16). That takes courage, to be honest – your editors might be completely, idiotically wrong about a paper. And it takes guts for the authors to put their ideas down, as they might also be utterly wrong (17). But this is the style of leadership needed. Genuine creativity and innovation comes from an innovator who is enabled to take a bold step, a giant leap, and has the courage to do so.

I hope that Bioscience Hypotheses has enabled some innovators to do this, and that Medical Hypotheses will continue to do so in the future.

REFERENCES

- 1) Bains, William. *Venture Capital and the European Biotechnology Industry*. 2008. Palgrave Macmillin (Basingstoke, UK)
- 2) Kealey, Terrance. *Sex, Science and Profits*. 2008. William Heinemann Ltd (Portsmouth, NH, USA)
- 3) Charlton, Bruce G.. Invisible colleges, private patronage and commercial profits versus public goods, government funding and ‘crowding-out’: Terence Kealey on the motivations and incentives driving science. *Medical Hypotheses* 2009, 72(2): 111-115
- 4) Charlton, Bruce G., Andras, Peter. ‘Down-shifting’ among top UK scientists – The decline of ‘revolutionary science’ and the rise of ‘normal science’ in the UK compared with the USA. *Medical Hypotheses* 2008, 70 (3): 465-472
- 5) Fairtlough Gerald. *Creative Compartments: a design for future organization*. London, UK: Adamantine Press; 1994.
- 6) Charlton, Bruce. False, trivial, obvious: Why new and revolutionary theories are typically disrespected. *Medical Hypotheses* 2008, 71 (1), 1-3
- 7) Lapidot, T., Sirard, C., Vormoor, J., Murdoch, B., Hoang, T., Caceres-Cortes, J., Minden, M., Paterson, B., Caligiuri, M. A., and Dick, J. E. A cell initiating human acute leukaemia after transplantation into SCID mice. *Nature*, 367: 645–648, 1994.
- 8) Griffin JD, Löwenberg B. Clonogenic cells in acute myeloblastic leukemia. *Blood*. 1986 68(6):1185-95.
- 9) Park, C. H., Bergsugel, D. E., and McCulloch, E. A. Mouse myeloma tumour stem cells: a primary cell culture assay. *J. Natl. Cancer Inst.*, 46: 411–422, 1971
- 10) Dick, John E. Breast cancer stem cells revealed. *PNAS* 2003 100(7), 3547-3549
- 11) Singh; Sheila K., Clarke; Ian D., Terasaki; Mizuhiko, Bonn; Victoria E., Hawkins; Cynthia, Squire; Jeremy, Birks; Peter B. Identification of a Cancer Stem Cell in Human Brain Tumors . *Cancer Research* 2003, 63, 5821-5828
- 12) Al-Hajj; Muhammed, Wicha; Max S., Benito-Hernandez; Adalberto, Morrison; Sean J., Clarke; Michael F. Prospective identification of tumorigenic breast cancer cells *PNAS* 2003 100:3983-3988
- 13) Yamanaka; Shinya, Takahashi; Kazutoshi Induction of Pluripotent Stem Cells from Mouse Embryonic and Adult Fibroblast Cultures by Defined Factors. *Cell* 2006 126 (4), 663-676

14) Okita; Keisuke, Tomoko Ichisaka; Tomoko, Yamanaka, Shinya. Generation of germline-competent induced pluripotent stem cells. *Nature* 2007, 448, 313-317

15) Bains, William. Many minds make light: Ideas and entrepreneurship in the life sciences. *Bioscience Hypotheses* 2009, 1(2): 65-66

16) Bains, William. Evolution of a Journal. Editorial Policy 2.0
Bioscience Hypotheses, 2009. 2 (2): 57-58

17) Bains, William. Hypotheses and humility: Ideas do not have to be right to be useful. *Bioscience Hypotheses* 2009, 2(1) 1-2

FIGURE 1.

Response to questionnaire 1) did you know that 2009 is the "European Year of Creativity and Innovation?", 2) If you did, what difference has it made to you?. Responses are categories by the main, recent employment of the respondent: 'Academic' – UK academic working in life sciences, 'Investor' – working for a venture investment group with specialism in early stage science or technology-based companies, 'SME' – working for a small, science- or technology-based company (including working as founder), 'University' – working for a University in a role relating to technology transfer, licensing or management, 'Support' – working in a support profession – patenting, law, journalism, technical or business consultancy - with a specialism in supporting small, technology-rich start-up companies, 'Other' – variety of other roles with direct connection to innovation in academia or industry.

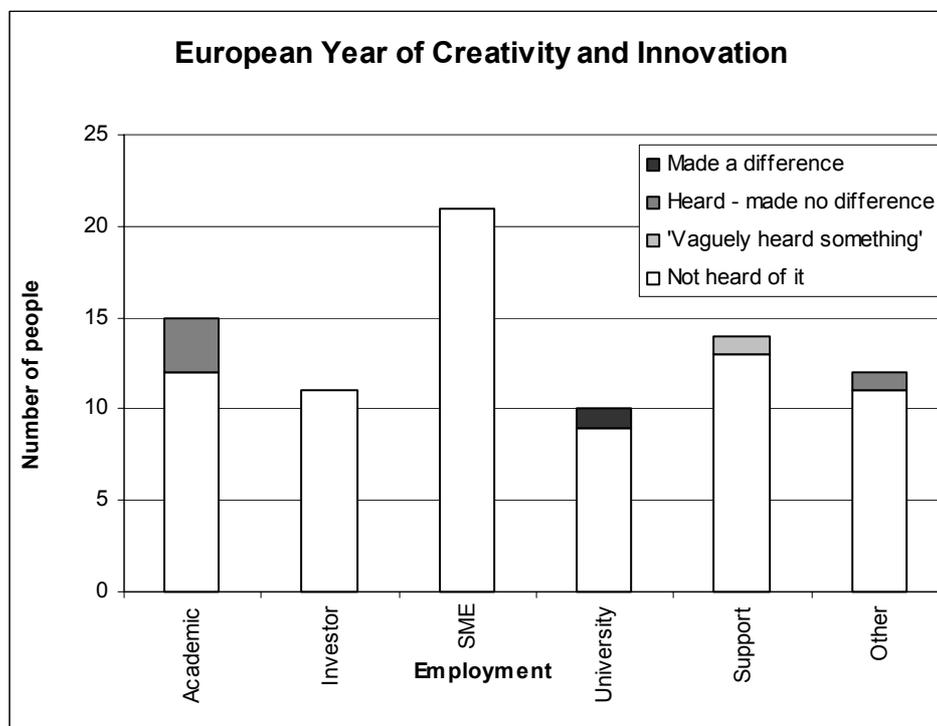


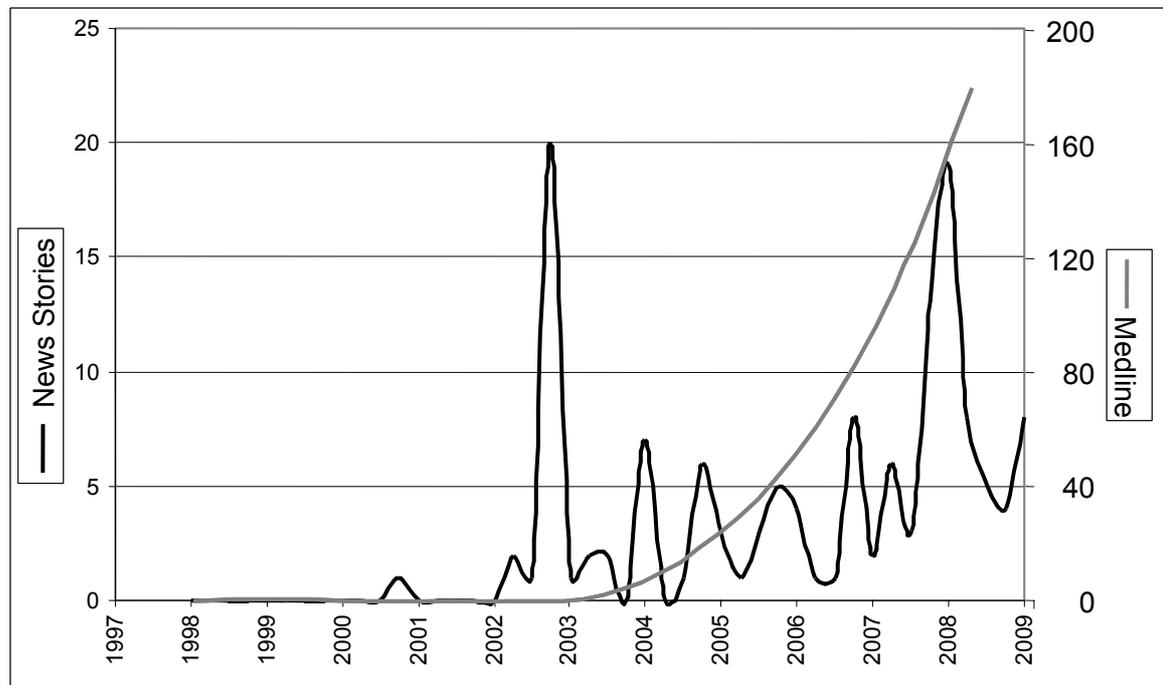
FIGURE 2

<Dilbert cartoon from 18th April 1998>

<http://www.dilbert.com/strips/comic/1999-04-18/>

FIGURE 3

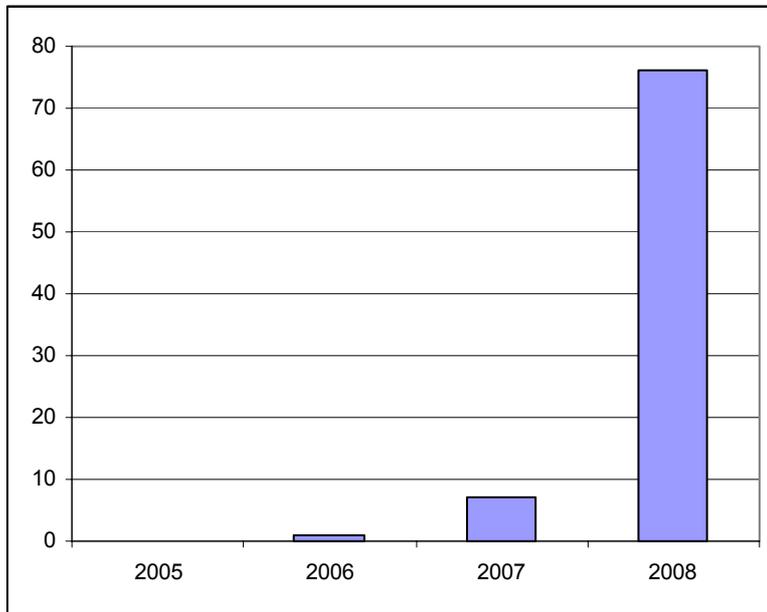
News stories and Medline articles about cancer stem cells.



News stories from Factiva (stories with 'cancer stem cell' keyword under Health/Living/Lifestyle/Science/technology, Market/Financial, or corporate digests.sections). Medline data from PubMed via Dan Corlan's search engine <http://dan.corlan.net/cgi-bin/medline-trend>

FIGURE 4

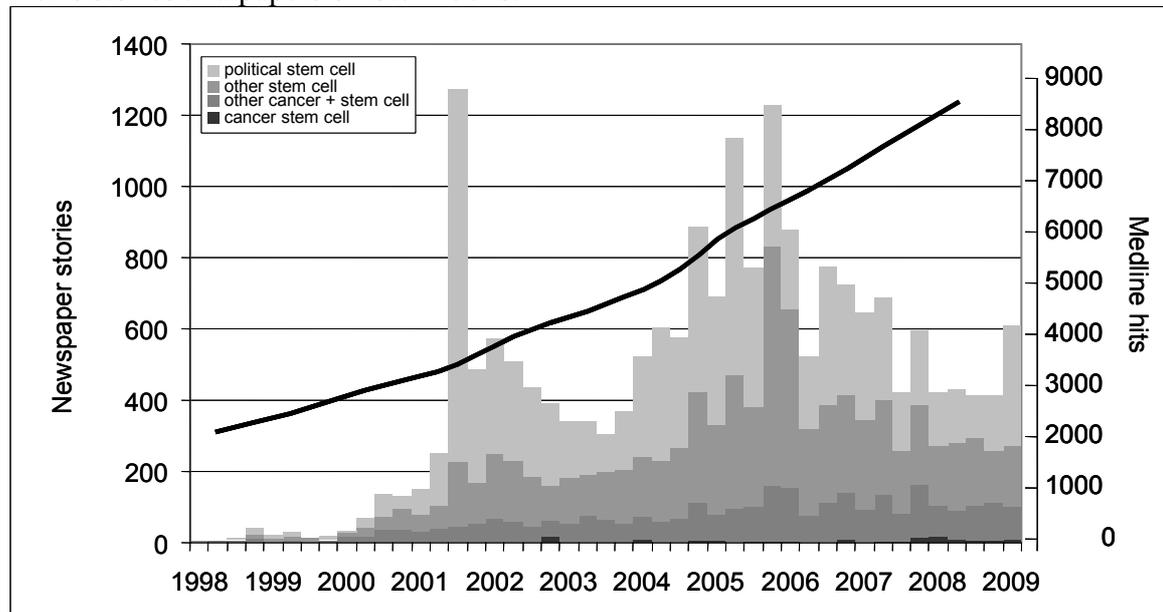
Medline papers on iPS cells.



Data from same source as Figure 2.

FIGURE 5

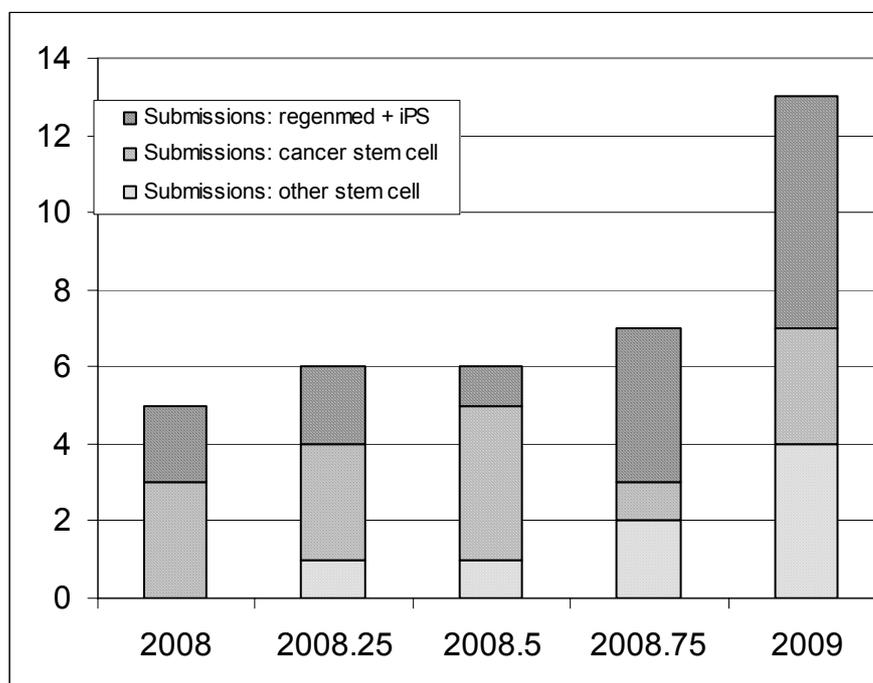
News stories and papers on Stem Cells.



News stories (vertical bars – left-hand axis) ‘Political stem cells’ – stories with key words relating to stem cells linked to words relating to US or religious controversy surrounding stem cells. All other do not feature political or religious keywords. ‘Cancer Stem Cells’ – stories with the specific term ‘cancer stem cell’. ‘Other cancer + stem cell’ – stories linking cancer and stem cells. ‘Other stem cells’ – all other stem cell stories. Papers (solid line, right-hand axis). Data from sources listed in Figure 2.

FIGURE 6

Submissions to Bioscience Hypotheses, by subject area.



Submissions in the stem cell area. 'regenmed + iPS' – regenerative medicine technology, and use of iPS cells. 'cancer stem cells' – cancer stem cell biology, including new suggestions for cancer treatment (which Bioscience Hypotheses does not publish). 'Other stem cells' – all other stem cell biology or therapeutics submissions.