I have been thinking more seriously about choosing a topic for a paper recently. You can't publish on something that has already been done though, so you need to pick a sub-field and review most or all of the existing papers to find gaps. Compiling a list of relevant papers and reading them is a painful but necessary task. It is also extremely discouraging. One quickly gets the impression that everything has been done already. All of your ideas have already been published, which has a way of trivializing them. It becomes clear that where there are "gaps" in the literature, they are quite small. Thus, it seems that the job of the researcher is, through a mix of brute force and sophistry, to pry open a gap and inhabit it. Or perhaps the task is to shrink yourself and your intellectual life down to the size of the gap. There is something absurd about all of this. It is clear that this is a certain type of game, driven not by concern for solving problems of practical importance, nor by a desire to educate effective stewards of scientific knowledge, rather by the single-minded pursuit of novelty for its own sake. But all of this drooling over The New is couched in bold claims of transformation and pragmatism. *How important could a problem be if one must toil to even identify it?*

To be clear, I am not the kind of person that thinks all learning should be in service of immediate practical ends. Quite the contrary, in fact. But if there are worthwhile goals other than the pragmatic, we should at least be honest about this. No need to pretend that we, as a community of students and researchers, work humbly at the behest of society. I don't mean to say that all academic work is useless, rather that at best, most fields are in a gray area. I know less about sociology, economics, psychology, etc. but in engineering this gray-ness is pretty obvious. There is value in playing intellectual games though, and my concern is that too extreme an emphasis on novelty in academia minimizes the social, psychological and political collateral benefits that a well-designed game is supposed to foster. For viewers and players alike, basketball would be a pretty underwhelming sport if the goal was to be the best teammate possible. The goal of winning games is chosen to maximize collateral benefits of what is at face value a pointless activity. And because this is a good goal, it also incentivizes other desirable behavior such as being a decent teammate. What are the collateral benefits of a game like this? There are many: competition which fosters camaraderie, examples of the fruits of discipline and commitment, a sense of purpose, a connection to place, rituals around spectatorship, etc. In my opinion, games are relatively pointless activities with desirable side effects. And I think academic research is often no different. Unfortunately, it has pretensions to its products being of explicit social importance which limit these desirable side effects. I think the importance of academia is quite more implicit than we realize.

Before going any further, I should give due credit to arguments in favor of novelty as the goal of science. Many of these ideas are from the articles whose links I have included below. The first argument has to do with the lack of financial incentives for research. Because they are made available through publication in journals, scientific findings are a public good. A research finding is often not the kind of thing one keeps secret and exploits for financial gain. Thus, there is not a strong market incentive to move one's field forward, but the issue can be remedied by showering researchers producing novel results with prestige and career opportunities. Social, not financial, incentives motivate the academic to unearth new public goods in the form of research, so the emphasis on novelty compensates for the inability of the market to incentivize scientific progress. Similarly, some argue that prioritizing novelty helps optimize the distribution of labor among research problems. To see this, note that the prestige awarded to an academic for a research finding is something like (novelty of finding) / (number of people working on the project). Though discovering a new particle at the CERN supercollider may be extremely novel, the fact that thousands of researchers were involved effectively waters down the acquired prestige of any given individual. The incentive of maximizing expected prestige, in which novelty plays a central role, effectively distributes labor among far-fetched questions, which are high-risk-high-reward, and “safe” projects, which are low-risk-medium-reward. In other words, academic institutions encourage scientists to maximize the following quantity:

(likelihood of success) x (novelty of finding) x (percent contribution to such finding)

This makes some sense. It helps to distribute labor among high- and low-risk research, and encourages scientific advancement in a way that the market does not. Lastly, we can note that a valid and common form of novelty in science is expanding a past work into new territories. This is science at its most incremental. This might be interpreted as a good incentive too, because it rewards scientists for replicating other people’s work as a potential basis to build off of. This scrutiny is an effective tool for weeding out bullshit.

I think these are some of the ways in which an emphasis on novelty in academia is a very effective tool for knowledge expansion. But in my opinion, this breaks down when novelty is the only goal, especially for mature fields in which outstanding problems are marginal. I would argue that scientific findings are a true public good (as in, something good for the public) only a small fraction of the time. This is blindingly obvious in quantitative sciences. If you disagree with this, go to any physics, math, or engineering department and ask graduate students: does your work benefit society? My guess is that common answers are somewhere between “no” and “no, and my work actively harms society.” I think we forget that a well-educated scientist is a public good independent of their research. It is an asset to society to have examples of individuals who choose to pursue knowledge, contemplation, and intellectual rigor. It is an asset to this person’s community to have access to an intellectual authority in their field. This person should hopefully be able to give humble and thoughtful opinions on matters personal and political, informed by their studies. I think we forget that graduate education is not simply a means to an end of producing new knowledge, it is a means of forming citizens. This aspect of becoming a scientist or researcher is totally overlooked by the goal of novelty. The social benefits of science come not only from research findings, but from the contributions to communities, small and large, of the scientists as individuals. And when one is constantly told to scramble to invent problems in fields which are already quite effective at addressing their core concerns, there is time only to educate oneself as a game-player rather than as a citizen.

The Palladium article I included says something like: the more closely social status corresponds to activities that are beneficial for society, the more such activities are incentivized. Right now, it seems that academic institutions reward researchers who spend their entire career engrossed in the abstract world of research. If they were pumping out cures for our besetting social evils, great. But this is not common. Where are the rewards for being deeply knowledgeable about one's field? Rest assured, this kind of knowledge only loosely tracks with being an engine for novelty. Where are the rewards for being an effective communicator of knowledge, and mentoring the next generation? Where are the rewards for working closely with students, to shape them as both scientists and humans? Where are the rewards for focusing on problems of real social importance? What about being a well-rounded citizen? It is disappointing that these things, which I see as the collateral benefits of science, go more-or-less unseen by the reward systems of academia. Maybe an emphasis on novelty is carry-over from a time when there were many new and deep truths to discover about the universe. Or maybe it is simply a lazy virtue, where instead of crafting a meticulous vision of what an embodiment of knowledge and wisdom look like in the 21st century, we default to The New being good enough.

Some articles I read:
https://www.ncbi.nlm.nih.gov/pmc/articles/PMC5526661/
https://letter.palladiummag.com/p/science-needs-sovereigns-44d
https://www.jstor.org/stable/26551882