Opinion

Freeloading Research

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With a so-called once in a generation refresh happening in our research system, and a government-led drive for research that delivers 'impact' for the economy I was piqued to read that Peter Hegseth had accused our wee nation of "freeloading" due to the small spend on military relative to GDP (MBIE, 2026, One News 2026). I won't pretend to be well abreast of what we should be doing with our military spending, although I will note that in the latest budget significant funding has been provisioned (Treasury, 2026). Rather, I was intrigued by what would happen if we considered our spending on research from a similar perspective. Are we setting ourselves up to be freeloaders in that context too?

Research (i.e. the generation of knowledge) is complex, and the nature of research can be discipline dependent, what is fundamental in social sciences or engineering might look less so to someone in physics or mathematics. Nonetheless, I don't think it is an over-simplification to divide research into 'basic' research, the type of research that aims to find out how the world (or systems within the world) work, and 'applied' research, the type of research that seeks to use knowledge of how the world works to solve a problem. Filling the apparent void between the two is a spectrum from 'basic' to 'applied', and no doubt there are people that will find this statement too broad for their taste, but I think that in most cases it is safe to characterize economic impact as belonging to the 'applied' end of that spectrum.

In my mind the work of Ernest Rutherford provides a nice relatable example of this distinction. While famous for his experiments in which he 'split the atom', more broadly Rutherford's work was pivotal in characterising the nature of radioactivity, and I hope readers will understand that this falls firmly in the "basic research" category above. One simple example building off this research is the humble smoke detector, making use of very small amounts of radioactive elements and the energy they emit to detect smoke particles in the air. The development of the smoke detector is a nice example of 'applied' research, building off the 'basic' research conducted by Rutherford. Now, I can't see back into the mind of Rutherford as he was pondering the nature of radioactivity, but I don't think it is too bold to suggest that he probably wasn't anticipating the development of the smoke detector as he developed his experiments to determine the nature of the mysterious energy emanating from radioactive elements. At least if it had occurred to him, I have never seen it characterised as a primary motivator. This was research where the main 'outome' was to explore the nature of the world around us and answer fundamental questions about how it worked. While in general terms we often anticipate that this research will have a positive impact on humanity, and therefore it is worth funding for the eventual benefits it will bring individual nations – a direct economic benefit is not typically tied to 'basic' research.

I myself am a chemist interested in "organic synthesis" and "medicinal chemistry". The former deals with the "how do we make things?" side of organic chemistry, concerned primarily with the types of molecules found in living things. It is largely focused on understanding how different combinations of chemicals can interact to make other new chemicals. Through sequences of these combinations we can build progressively more complex chemical structures that may have some utility. Decades of research around the world has established a large body of knowledge that describes how certain families of chemicals combined under certain conditions produce predictable downstream structures. Alongside this, research has also developed models of the movement of subatomic particles (like the ones Rutherford was interested in) that we use to rationalise why certain combinations of chemicals provide certain outcomes. There is often a motivation for why we want to make certain types of chemical, so I would say this is further along the 'basic' – 'applied' spectrum than Rutherford's work on radioactivity, but in general I would still class this as 'basic' research.

The other arm of my interests is medicinal chemistry, and this is in essence the application of synthesis to a specific problem – treating a disease. This work is clearly more 'applied', and it also has more relevance to economic benefit, although that benefit is typically a distant horizon as development of pharmaceuticals is a notoriously long process (Brown et al., 2021). Work in medicinal chemistry necessarily relies on the insights gained from synthesis research, without it we simply wouldn't have the tools to develop new drug candidates. Hopefully it is evident, that in the case of smoke detectors and new pharmaceuticals researchers must rely on the body of 'basic' research produced over time without the burden of direct economic

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impact.

To tie these things back to Peter Hegseth's comment about freeloaders we have consider how research is funded. In most cases researchers will write an application saying what they wish to do, how they're going to do it and why they're the right people to do it. This goes into a competitive process and if they tick the right boxes, they might get funded to do the work. Historically we have had a system with different funding for 'basic' research, and 'applied' research with the selection processes designed to understand the different motivations of the two. There have always been the awkward projects that sit somewhere on the spectrum, representing a challenge to the researchers, but generally it is accepted that both ends of the spectrum need to be supported. Recently the government has instituted changes that convert our research system to one that favours economic impact. This has been carried out through pathways such as directives to fund managers, an explicit change to research strategy and new directives issued to research institutions (MBIE, 2026, Ministry of Education 2026).

In recent comments to BusinessDesk the Prime Minister's Chief Science Advisor John Roche said "We do great science, but the delivery out of the end of that pipeline hasn't been as great as what it could be.", a statement I'm inclined to agree with, and it appears that we also agree that 'blue-sky' research (a term commonly used for 'basic' research where the future use case is not apparent) is important, so far – so good (Hurrell, 2026). He then goes on to criticise the previous 'basic' funding model for not pushing researchers to talk with "people in the mid-technology readiness area" and later characterises the Government's intent to "invest in outcomes", not "fund research projects" as scary for some researchers. I tend to agree, it is scary. While at face value it might sound like a good strategy, investing in outcomes makes sure that we pursue research that will be productive for our economy, it also seems to block research that aims to discover knowledge where the future use case is not apparent.

Imagine Rutherford trying to describe the potential economic impact of understanding radioactivity - sure, he could've made a guess, but anything too specific would have been disingenuous. Similarly, if I do work to understand how a new combination of molecules forms a specific structure I could make some sweeping statement about future economic impact, but anything too specific and I would be similarly disingenuous. Unfortunately, in many cases this is what is being asked of researchers who have aspirations in 'basic' research, and for most researchers winning research funding is a major factor in staying employed.

For me, the logical choice is to therefore focus my efforts on the 'applied' work or medicinal chemistry which should be more fundable in this system because I can point to a specific economic impact. In doing so I would be building off the past 'basic' research work of others, while personally and systemically not contributing to 'basic' research.

Would freeloading by any other name smell less sour?

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