

TRANSCRIPT OF DEVELOPMENT DRUMS [EPISODE 28 – ADAPT, WHY SUCCESS ALWAYS BEGINS WITH FAILURE]

Host: Owen Barder. Guest: Tim Harford

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Thanks for downloading Development Drums number 28. I'm Owen Barder, of the Center for Global Development and my guest today is Tim Harford.

Tim is a writer at the Financial Times, known especially for his Undercover Economist column and his best-selling book of the same name. You may also know Tim from, if you're interested in the aid industry, from his volume *The Market for Aid*, which he wrote with Michael Klein when he was at the IFC, or for his book *The Logic of Life*. Today Tim is at CGD to talk about his new book "Adapt, Why Success Always Starts With Failure."

Tim, welcome to Development Drums.

Tim Harford

Thank you very much.

Owen Barder

First, let me plug your book. I thought it was really good. It's one of those books that makes you think differently about the world by taking a simple idea and showing how it applies in lots of different aspects of life. So let's start with main theme of the book: that we achieve difficult things when we plan and expect to fail. At one point you set out something you called "Peter Palchinsky's principles". So who is Peter Palchinsky, and what are his principles?

Tim Harford

In a nutshell Peter Palchinsky is the engineer who told Stalin that he was wrong. And you can guess what happened to Palchinsky. So Palchinsky was actually a compulsive truth-teller. He told to Czar that he was wrong. He composed a letter to the Supreme Soviet explaining that science was more important than socialism, fortunately his friends persuaded him not to send it. He toured Europe, learning all about engineering, and while he was doing that he had an affair and wrote back to his wife and said "by the way I had an affair, hope that's cool with you"... I mean, he just couldn't stop telling the truth, and in the end he was killed by Stalin.

So he is a tragicomic figure. But one of the things I learned from Palchinsky is that the nature of his criticisms of Stalin's grand engineering projects were – pointed me in the direction of successful principles for adapting. And actually the same thing that you would get if you study evolutionary biology, you would get the same thing out.

You need lots of ideas. You need to be trying also different things, because things fail a lot. And the second thing is that the variation needs to be safe. You can't be trying ideas that will finish you off if you – if they fail. Peter Palchinsky didn't take his own advice on that. But he would recommend you – faced with some project to build a gigantic dam, he would say, well, actually we don't really know a lot about the geology of the region and maybe we should build a small dam first, and we don't need a big dam yet. Let's build several small dams, that kind of thing.

So you need to do it small, because things go wrong. And the third thing is selection. You need to know the difference between what's working, what's not. And the tragic coda to Palchinsky's life is the very first project he did was to study the working conditions of miners in the Don Basin and he explained to the Czar that these miners were very, very badly treated and needed much better conditions. And that was not a welcome message.

When the Soviet Union collapsed, we all noticed things like the fall of the Berlin wall, but one of the key events in the collapse of the Soviet Union was a huge piece of industrial action, a huge miners' strike, which forced Gorbachev into humiliating climb-down. And it was a key moment, the first ever major industrial action in Soviet history. It was 90 years after Peter Palchinsky had first commented on the coal miners' conditions and it was the same – it was the great grandchildren of the original coal miners, in the same conditions. So this was a system that was unable to adapt.

So just to summarize those three principles again: you need to try a lot of different things; they need to be small enough that failures will not ruin you; and you need to be able to distinguish success from failure, which some systems are very ill-equipped to do.

Owen Barder

The one thing I didn't expect when I picked up the book was that one of your examples would be from the military. You – I've lazily tended to think the military is being very command-and-control, very top-down, not very prone to experimentation and the kind of place where failure is problematic, rather than something that you can plan for and accept. But you tell us in the book that actually the best military strategists actually adopt some of these principles. What are the examples of that?

Tim Harford

Yes, the reason I look – it was a surprise to me as well. The reason I looked at the military is because originally *Adapt* was not a book about trial and error, it was a book about how complex problems get solved. And so I looked at the war in Iraq because it seems to me to be an important complex problem. It was only as I looked at all of these different complex problems: poverty, climate change, innovation, financial crisis, Iraq; I realized actually that they were all getting solved because of trial and error. Or not getting solved because we don't have processes for trial and error.

So that was – it was a surprise to me as well. And what I learned looking at Iraq was the narrative that we tell is: things got really, really bad. This operation was entirely botched, the strategy was not working, Iraq was on the brink of civil war. And we had a bad leader, and we got rid of the bad leader and we got a good leader. Donald Rumsfeld was removed from office after the voters registered their disapproval. General David Petraeus took over in charge of the operations in Iraq, and General David Petraeus was a good leader, and he fixed it. And that is the sort of narrative that we tend to tell ourselves, about how any problem gets fixed. And that's not happened. What happened was that the Colonels and the Majors of the US Army, at tremendous risk to their own careers – and there were career consequences for them – rebelled against the strategy they were being forced to implement.

Because they could see on the ground, taking what Muhammad Yunus, in a totally different field calls “the wormseye-view”. They could see what was happening, they could see the strategy was failing. And they were trying to figure out what else they might do. And of course different things were tried, and not everything worked, but the successful experiments on the ground were copied, were spread through the lower ranks of the army. It was like schoolboys hanging – handing dirty magazines around – “we found some tips to deal with the insurgency. Don't tell Donald Rumsfeld. Donald Rumsfeld doesn't even want us to use the word insurgency”. This went around.

David Petraeus is a great leader. Not because he fixed the thing from top down, but because even before he was in charge of Iraq, he recognized that this was going on. He pulled all this stuff together, he turned it into a new doctrine for the American Army. He launched a media campaign, it got reviewed in the New York Times book review, it was on Charlie Rose and when everyone was talking about this new approach to counter-insurgency. The US Army had changed itself from the bottom up. Petraeus was helping to capitalize that. When he came into the top, the job was already half-done. And in a nutshell it's what one

British general told me, he said: “We obviously and readily learn lessons from the front line because they save lives. At the lower levels of the army that’s where we learn the lessons, but at the top it’s much much harder and we are much more reluctant to adapt”.

Owen Barder

It seemed to me, reading the military chapter, that there were in principle two distinct lessons that one could learn. One was the importance of, at the top adapting by learning and the problems they’ve had in doing that, and you gave the Donald Rumsfeld example. The other lesson in the book was the idea that you need to try to delegate decision making, and let commanders on the ground make their own judgments. And one of the tricks, one of the difficult things to know is to what extent do you need a top-down the strategy and to what extent do you want people in the field to be ultimate their own decisions, and where do you draw the line? And you give the examples in business of Whole Foods in the United States and Timpson’s in the UK that have done very well because they’ve empowered their staff to make their own decisions.

But that seems to be a slightly different point than your general narrative about failing and learning from failure and failing safely. The idea that actually you don’t want a strategy at all. So how does the idea of delegation fit into your overall story?

Tim Harford

It’s a good question. I think delegation is not always the solution, but it makes experimentation and trial and error – often makes it easier. One is it’s – you get more variation if there’s delegation, because different people are trying different things. You also tend to have a closer response to reality through delegation. So you’re selecting good ideas versus bad ideas because people see what’s working. So that’s why I think delegation tends to be helpful. You clearly don’t have to delegate to experiment. If you look at organizations such as the Cochrane Collaboration, which is a very, very important medical institution I suppose is the way to describe it. That is a database pooling together information about different randomized clinical trials that have been done in medicine and being very, very systematic about how we collect and collate this information rather than just saying to doctors: “Well just improvise, do whatever you like”. We actually need to be systematic about this. So delegation is not always the way forward, but it often helps.

One of the lessons from the book incidentally is: it’s not enough just to delegate, you also need some system of monitoring. And often the best people to monitor are peers. So your colleagues are the ones – and this happens in Whole Foods supermarket, it happens in the US Army, it happens in Google, it happens all over the place. Your colleagues are the ones who actually know whether you’re doing a good job or not. So if you’ve got some system that makes that peer monitoring effective, that really helps turn delegation and decentralization into something that will experiment in a productive way.

Owen Barder

But as we’re thinking about how we take your ideas and apply them in practice – and we will come to the development world in a second – understanding this “combination of” and a big part of the debate in development is how much top-down strategy do we have? And how much decentralization and delegation do we have? And it seems to me, if I’m hearing you right, that you need both and you need a feedback loop that connects what’s happening on the ground to the strategy, and that’s important. But I’m not sure that answers the question of how would we know if we had enough decentralization and delegation or too much, or what had – what kind of measures do we have of what the right amount is?

Tim Harford

That’s a good question and it’s not one I have an easy answer to. How would we know that we have decentralized enough? My instinct, looking at the aid industry, which I used to be part of, is that we – it is by nature a top-down industry. Ultimately the power comes from the donors. The donors are governments, they’re highly centralized institutions, that’s what a government is. And they’re dealing with other governments. And the narrative in the official aid industry tends to be one of harmonization, which – and I understand that fragmented aid and disorganized aid can be damaging. But we could imagine coffee industry getting together and trying to harmonize, so Starbucks and Cozy and all of these coffee chains getting together and saying, “Well we need to harmonize our offerings”, I mean, we should – they’d be

prosecuted. And we – we feel it – we feel quite happy with these people experimenting doing these different things because we feel that the feedback loops are there. There is a reward for doing things right and not doing things right. So the challenge for the aid industry I think is not so much to say: “Well you need to decentralize more or centralize more”, but we need to get the feedback loops in place. And if the feedback loops were in place to reward something that was working and to withdraw funding and effort and attention from things were not working. We would just be a lot less worried about our failure to coordinate, our failure to coordinate would be a strength rather than a weakness.

Owen Barder

I’m curious to get your sense of whether you’re saying something very different from, say Charles Darwin talking about biological evolutions, survival of the fittest. Or Hayek talking about markets. I mean, is the idea that what you need is experimentation and failure, and are you just saying that that turns out to be something that applies across a lot of different walks of life. So are we – are you simply applying Hayek and Darwin to more things, or you – is that something different, some broader point?

Tim Harford

I – no, I think conceptually it’s a Darwinian view.

Owen Barder

You would – you wouldn’t associate yourself with Hayek, just tell me...

Tim Harford

Well, Hayek as well, although I recently had some very bad news about Hayek’s political views I was not aware of, with his endorsement to General Pinochet, so I need to add a footnote to the second edition of the book, I mean, you – this is so... But Hayek too was all about decentralization and the evolution of effective systems. So in that respect I think the concept is not new. What is new in the book is to show how – to make the case this is very important in the economy, which a minority of economists make this case. I’m not the first person to do it. But it is not the majority view, certainly not reflected in economic models.

And furthermore to then say: “Well if trial and error, if evolution is so important, why don’t we do more of it and how does it work?” So the whole book is really examples, case studies, anecdotes, narratives, about trial and error either happening or not happening in all kinds of different fields. That was actually a challenge for me as a writer because I started off as an economist and naturally I’d say well, look here – I put the case to my publisher I said, trial and error is really important, we need to write a book about why we should all experiment more. And the first question that my editor had for me is, okay, well that’s fine so, what does it take, why do we find it difficult? And then that became – that turned out to be a really interesting idea: why psychologically, why is it hard, politically why is it hard? And it’s maybe not a coincidence that every chapter in the book has a hero, has somebody who actually has tremendous strength of character because our systems do not adapt and experiment very naturally: it actually takes very brave people to make some do so.

Owen Barder

Can we talk about climate change because there’s a key issue in development, where you argue very forcefully that the adoption of trial and error techniques is going to be very important and you have this lovely story of trying to do a planned response to climate change, where you’re trying to figure out what the most environmentally friendly action is at each point and how difficult that is? Tell us a bit about why you think that the trial and error approach would work well in climate change and what that means in practice?

Tim Harford

So if we think about the problem of climate change, it’s a very complicated problem and I made that point once at a meeting of activists and they were very unhappy that I said it was complicated because they thought that I was suggesting that the science was very uncertain and, you know, we couldn’t be sure it was happening. And clearly science is uncertain, but that’s not what I was making. That’s not the point I was making.

The point I was making is, what is our appropriate response to climate change? Well, it is largely trying to reduce emissions of carbon dioxide and to reduce the emissions of other greenhouse gases. And also to explore other adaptations, such as can we – is there a way to take carbon dioxide out of the atmosphere is there a way that we can adapt to the climate change, new crops, new ways, all sorts of different things and these basically involve all 7 billion people on the planet, changing in small ways or large ways, everything they do, every day. Every product that we consume, every action that we take has an impact on the planet. 7 billion people taking hundreds of decisions a day, that sounds like a complex problem to me.

And that's the nature of the change that we need to make. And it also strikes me as, it shouldn't be controversial, but it's something that people don't appreciate enough. So the dialogue on climate change, in as much we have a healthy dialogue at all, and clearly there is a lot of nonsense talked about it. In as much as we have a healthy dialogue at all, it seems to be all about making the moral case that we really need to redouble our targets and commit to ever greater reductions, in a very top-down way and it's never discussed exactly what policies you're going to put in place to make this happen. And when I look at the policies that are used, they're often very counter-productive. They – so just one simple example. In the UK we have the Merton rule. It's sprung up and it's been widely copied by local councils. It's a rule that you can't get permission to build a new development unless it has the capacity to generate 10% of its electricity on-site through renewable resources.

And it sounds like a really easy – it doesn't cost the council anything to make that rule, and so developers whenever they build a new building they have to install some renewable capacity. It sounds like a great idea, who could object to that? If you look at the effects, so I talk to engineers and architects, who have to live with this rule. So one of them said, well I'm trying to build a skyscraper in Central London. The skyscraper is inherently a very environmentally efficient building; it is right next to Waterloo Station. People are going to come here by public transport. Then they're going to travel, there is a lot of office capacity and they will travel by elevator and elevator is an incredibly efficient machine because it's counter weights. If these offices were just out in rural Berkshire somewhere you'd have huge, huge car parks, everyone would be driving, so it's a hugely efficient concept, irrespective of whether there was any renewable energy onsite.

Now I need to satisfy the Merton rule. So I need to generate 10% of electricity onsite, but 10% of the electricity of a skyscraper is huge. And where is the space to – there is no space for solar panels, no space for windmills, it can't be done. So what you do? It turns out there is one solution, you have a wood-burning furnace in the basement of the skyscraper in the center of London and you drive trucks twice a week and dump wood pellets in a storage chamber, the size of a swimming pool and that satisfies the Merton rule.

Meanwhile, you have an out-of-town supermarket, loads and loads of space for solar thermal panels, for windmills, for heat pumps under the car parks that can cool and heat them, incredibly efficient, you could generate electricity from this site because of the nature of the site. It would be tremendously useful, but the Merton rule basically makes none of these demands, it says I will just put up some token, irrespective to the fact you could generate all the electricity here, just put up some token, piece of renewable energy. And by the way the entire building's an environmental disaster because it's an out-of-town shopping center, so it's just one example of rules that we impose that seem sensible, but given the complexity of the economic system, they're not really helping us respond effectively.

Owen Barder

So in that case the problem is that everybody is having to do the same thing whereas it makes more sense to do it at the supermarket than it does to do in the skyscraper and that's why you come down to the idea of carbon taxes or a cap and trade ...

Tim Harford

Yeah, which I regard as effectively equivalent. I mean, there are some small differences between cap and trade and carbon taxes, but actually they both put a price on carbon, they both basically do the same thing.

Owen Barder

Right. We could have a separate discussion about that, but let's – take that for granted, that those are broadly the same idea, which is that you put up the price of the carbon and then you let those decisions percolate through the system, different people will find different ways of reducing their consumption of carbon or finding ways of generating clean energy. So a lot of people who are often to the left are very worried about the idea of using markets to solve these big social problems, that this is too important to leave to the market and that we need government intervention to make it happen. What's your answer to that?

Tim Harford

I don't think a carbon tax would solve the whole problem. I think we need some support for the innovation system because there are certain sorts of innovations that, short-term rise in the price of carbon is not going to suddenly generate very long-term transformative changes in the energy system. So it's not going to solve the whole problem. But basically it's a way of providing information to everyday people to make the decisions that they really ought to make for the sake of the planet. I have a little comic vignette of this well-meaning environmentalist in my book where he is trying really hard to save the planet, but he is – because he's just seen an Al Gore movie and he is convinced, it's really important, but he doesn't really know what he is doing and he just makes all kinds of mistakes. He is doing things that seem sensible, but actually counterproductive. So for instance, he decides not to have toast in the morning because the toaster consumes electricity so he has milk on his cereal instead, but actually milk is an embodiment of methane, because you can't produce milk without a cow and cows produce methane and methane, out of their mouths by the way just to be clear. And methane is a very powerful greenhouse gas. And actually it would have been much better if he'd just stuck to the toast ...

Owen Barder

And I think you say there's more carbon emissions embodied in a piece of butter on the toast than there was in toasting the toast.

Tim Harford

There is. Yeah, I had a very smart research assistant who calculated that for me and I trust her on that one, she came up with this wonderful factoid, so yeah, the toast is less of a problem for the planet than the butter on the toast.

Owen Barder

So this again is an example actually of the idea that we need to decentralize decisions and ...

Tim Harford

But with the information and the incentives, and at the moment we don't have the information and incentives. And the carbon tax would provide it. And a metaphor for the carbon tax is, imagine anytime you bought any product, or took any action, you pointed a smartphone at the barcode and got a little readout that said, this is the carbon dioxide, the methane and so on embodied in this product. And effectively a carbon tax is doing that. It's like a huge cloud computer that is calculating the implicit cost through the market system. And as an extra bonus, it not only gives you the information, it actually gives you an incentive to respond to the information because if you can do something, whether it's moving to an apartment closer to where you live, or drinking espresso instead of cappuccino because it's less milk, I mean it could be anything that you do. You are being given an incentive to make those decisions, and you decide how much you really value the cappuccino, whether you want to keep going or all, we don't want the government to ban cappuccino, but we do want people to know it's actually a more environmentally damaging drink than say an espresso is.

Owen Barder

So let's switch to the broader development questions and a big issue with development at the moment, all the rage is this question of impact evaluation, and in particular the use of randomized trials to test what kinds of policies and interventions work and what kinds of things don't work. And you have an example – this is an example in your book of where trying things to see what actually works in education, and your particular example gave this information we didn't have. Tell us about what you learn from that?

Tim Harford

So this is almost the canonical example of a randomized trial in development run by Ted Miguel and Michael Kremer in Kenya a few years ago. And the basic idea was funded by a Dutch NGO. We would like Kenyan children to be better educated. Well, what we do? Well, why don't we just give them textbooks. It seems like a perfectly reasonable idea. Now what Kremer and Miguel did was to say well, let us get a survey, a group of worthy schools for this project, make sure that there are more schools in that pool than you could afford to actually supply with textbooks. So maybe you can supply 100 schools, we will choose 200 schools.

Now all those schools are equally worthy, we will randomize which of the schools get the textbooks and which don't. There is no ethical problem with that because there were only ever 100 schools going to get the textbooks. And what could be fairer than a lottery, if you have a scarce resource it's perfectly fair. We will hand out these textbooks and then we will go back, we will find out in a year's time whether it helped. Very simple idea and just really making a virtue of necessity and creating a valuation, where, at fairly low cost where one previously didn't exist. They went back and they discovered all the textbooks were no use.

Owen Barder

They didn't make a difference to...?

Tim Harford

They didn't improve test scores for instance. The – I mean I forget the details about exactly in what respect the textbooks were no use, but they were regarded as a disappointment and they – I think they helped a few of the very smartest children. And it made sense because I think these textbooks were written in the third language of these rural children. So it's a big ask to make them work. So then they adapted this and said we're going to have a new project and we're going to supply beautiful colored flip charts, geography and biology and so on; teachers will be able to use these flip charts and they'll be friendlier for the children and so on. So there's a great example.

But rather than just saying well that's obviously the solution, they did the same thing again, another randomized trial, they came back. The flip charts didn't work either. What eventually did work was actually worming tablets, treating the children for tapeworm because these children are malnourished. They're missing school, or they can't concentrate in school because they don't have enough nutrients because the tapeworm is parasitic and is consuming a lot of nutrients. And tapeworm tablets were very cheap and very effective and so this lesson was learned.

And what could have regarded either as a success, but wasn't a success, and the NGO could have just distributed leaflets about all the wonderful textbooks they were giving to kids. And I mean that is how a lot of development organizations raise their money. To their credit they didn't do that, neither did they regard what they had done as a failure, they said, well we didn't fail, we learned something that didn't work and that's a very valuable piece of information. There was a coda to this. Well there are – I'd guess there were two codas. So, one is that the idea was then rolled out in India and they tried to adapt it to local circumstances and said well actually the problem here seems to be anemia, so we're going to treat the children for anemia with iron tablets rather than tapeworm. So the same basic concept, but adapted to local circumstances.

That said, I'm not aware of the follow-up study being published for that original piece of work and I think it should have been published by now. I'm not aware of sufficient replication that has gone on. And so this is a problem because while randomized trials are getting really a lot of traction, I think justifiably because they're very important. We still don't have the institutions necessary to sustain, say replication attempts or trial registries. Or the sorts of things that very, very slowly and painfully have caught on in medicine because we realize they're very important for getting the most out of randomized trials. So it's a great step, new step in the right direction, but we need to do more.

Owen Barder

Part of the reason for that of course is that academics have an incentive to develop new and interesting techniques. But once they've developed it and proven it and published the paper about it, it isn't their

business to then replicate it and use the same technique in lots of other countries and check whether you get the same results because their incentive is to develop interesting new academic surveys and it ought to be somebody else's job to figure out whether these same results apply in other contexts and scale it up and of course what happens in health is you have to do randomized control trials because otherwise you can't sell the drug.

Tim Harford

Yeah.

Owen Barder

But there isn't a requirement in development that says you have to do a randomized control trial. So on the whole we don't.

Tim Harford

And that's absolutely true. And I suspect it's impractical to insist on a randomized control trial for all development projects, that social projects are harder to evaluate than medical treatments. So there is a reason why randomized trials are less popular. But there is, I mean, the chapter in the book where I talk about randomized trials in development, I know there are two excellent books now out about this. Poor Economics and More Than Good Intentions. I recommend them both.

This – the chapter in my book, what I try to add that you will not get for example from Esther Duflo or Dean Karlan, is looking at what happened in medicine and the evolution of randomized trials, the ethics of randomized trials and then the struggle that their proponents faced to introduce them in medicine because actually I think that's – there is a tremendous amount to learn. And one fascinating thing that is true of medicine, and I think is not sufficiently true to development economics is, where were doctors trained? They're trained not in universities, but in university hospitals. They're trained by practicing doctors who are also researchers.

The researchers are producing academically rigorous research that is informed by their own clinical practice. And doctors when they go on to be practicing doctors get the British Medical Journal regularly summarizing all latest research. So there's an incredibly tight loop between research and practice. And I don't think we should simply say it's the responsibility of the academics to do the evaluation and then the World Bank should pay for the replication maybe. I mean, maybe the World Bank should pay for replication, but I don't think it's simple as that. We need a tighter loop between research and practice in development economics.

Owen Barder

That's a very interesting insight. Now, one -- so one of the critiques is about the ethics of it and you've given a convincing argument that says, 'well, we have to limit who we can provide these things to' and the point you make in the book is 'surely it's also unethical to go on doing things without ever finding out what works and what doesn't work'. But there is another critique of this approach, which is the Lant Pritchett point, which says that you – randomised trials work well for things like 'how do you make sure your clinic is properly stocked?' or 'how do you get more kids to go to school?', but it doesn't answer the big questions in development, which is 'how do you bring about institutional change? How do you bring about the introduction of more democratic and accountable governments?' Those are the big fundamental shifts, how do you start up a process of industrialization and the economic growth? And I think his quote is that we end up – it's a cul de sac of precise answers to trivial questions and that it doesn't address the big questions. What I think Esther would say – 'well no, these smaller questions add up to a big picture', but where are you on that?

Tim Harford

So I do not sympathize with the idea that these are trivial questions. So I was born in 1973, and like most babies born in 1973 in the West I was put on my stomach to sleep because that was the received wisdom of the doctors at the time. Benjamin Spock's Baby and Childcare book said this is what you should do, there was no contrary evidence. Gradually contrary evidence started to build – this is actually not the safe way to leave your children sleeping. It's a small effect, but over millions and millions and millions of babies it's

very important. And it took too long for the trials to be done and then too long for the information to be promulgated and to turn into practical advice. And a recent epidemiological study reckons 60,000 -- about 60,000 babies died as a result of this evaluation not having been done quickly enough and the information being released quickly enough.

So I am rejecting the idea that randomized trials study trivial problems. But you have to sympathize with Lant's broader point that, ultimately, what's going on in China for instance, is not because of randomized trials, and the rise of Europe, the rise of America was not the result of randomized trials. It was the result of much more informal and grander scale experimentations. The market system is very experimental. China was not a market economy, but China's reforms under Deng Xiaoping were highly experimental. The idea was 'well, we'll try this, that we will tolerate various experiments going on and we will shut down what's not working and we will copy or allow to be copied what is working'.

And that happened again and again and again. I mean, I described this in the last chapter of my first book, *The Undercover Economist*, it's a very experimental political system there in China, for all its faults. So towards the end of the chapter about development, I broaden things out. I talk about your work, Owen, on feedback loops and I talk about the general idea of how do we experiment and select good ideas on a bigger scale. This could be anything to do with proper evaluation of aid agencies -- who is doing good aid and who is not. Proper monitoring, transparency of aid flows or even some of the really grand stuff -- that Paul Romer's idea of charter cities -- ultimately, what is that but an experiment on a small enough scale to be survivable, but a big enough scale to make a difference? So randomized trials are great, but they are not the only kind of experiment and development needs a lot more experiments and a lot better feedback loops, which is a point you've made yourself very cogently.

Owen Barder

So one issue that a number of people have raised, particularly on the Development Drums page on Facebook about your idea that we need to learn how to fail, is the difficulty of failing safely in development. There's a very brittle, fragile consensus that allows us to go on spending development aid. And we feel at the moment as if to sustain that consensus we have to emphasize the success we are having, and that if we have too many public failures the consequence will be the public congressional support in the United States, parliamentary support, public support for aid, will decline.

And you have seen this a bit with the Global Fund who had a big audit process. They uncovered some quite small amounts, in the grand scheme of things, quite small amounts of corruption. They published that information and the consequence was that Germany, Ireland and Sweden cut their funding to the Global Fund. So if we have a culture in which when people confess to failure and try and learn from it, the result is their stakeholders back away from them, that may be good for the system as a whole, but it's not good for the individual who fails, right? This is your guppy example. The guppies evolved to acquire camouflage but that's no fun if you're the individual guppy that gets eaten. So, how do we learn to fail well in the development system in that kind of environment?

Tim Harford

Yeah. I have been thinking about this a lot, and I very much sympathize with this problem. If you think about the incentives in politics -- and development is politics -- they are very different from the incentives we have in markets, or in say, scientific progress. So in a market you have a hundred failures and one success, and if that success is General Electric or Google, then it's worth the hundred failures. Because those failures would all be on a fairly small scale. In science we have lots of failed experiments, lots of failed theories, but the whole scientific method, albeit it's imperfectly implemented, the whole scientific method is selecting for the stuff that is working. So again, you have a hundred failures, one success -- that's progress.

In politics, including in development, the incentives are reversed, you have a hundred successes, one failure -- and your political opponents will seize upon the failure and talk about that. And so I fully sympathize, but this is a big, big problem. So is there a solution? I think partly it's about creating a culture where you are very transparent and upfront about what you are setting out to do. So when you launch a new program, to say 'this is an idea we think may work and we are going to do a trial and we will see if it works, and if it

does not work we will shut it down and we will try something else' rather than say, auditing something and coming back and saying 'well, we discovered all kinds of problems'. I mean, besides, if you set up expectations in advance, that helps.

We also -- it would be just wonderful to be able to get away from the current debate in -- does aid work? I mean, I just find this debate so pathetic and so tedious, and sadly every book on aid, even some very good books starts with Bill Easterly vs. Jeff Sachs, and it's just so boring. Because what we -- the question that is being asked here without specifically -- I don't want to criticize Easterly or Sachs specifically, but the way the debate has turned into is just 'does aid work or does aid not work?'. This is an insane question -- it's like saying does...?

Owen Barder

'Does health work?'

Tim Harford

Yeah, 'does health work? Are teachers effective or are they not effective?' -- it depends on the teacher! It's just -- 'does software work or not?' -- it depends on the software. I mean it's a crazy question! And so we have to grow up and say we are going to try to discover which aid works and which doesn't. And I feel -- I understand the nervousness of demonstrating beyond doubt that some aid projects have failed. But I think we have to get to that stage and if we manage expectations in advance, it's something that we can do. And we just -- we owe it, as professionals in the aid industry, owe it to the poor people they are trying to help. We can't evaluate ourselves because we might embarrass ourselves. And, I mean, that's unsupportable.

Owen Barder

So randomized control trials are not the only way for aid agencies to learn and for the development system to learn. And it seems to me that in your appeal for more experimentation, more willingness to fail, more willingness to learn from that failure -- there are a range of things that we could do, which are more related to the experience of people on the ground about what's working, and what isn't working. The RCTs have the slightly top-down characteristic that you -- you have researchers from MIT, or from Yale, who come and test whether it worked or not. What are the ways that we can experiment and fail and learn that don't involve RCTs?

Tim Harford

That's a very important because there's so many things cannot be tested with RCTs. Google did not come about because of a randomized controlled trial, General Electric was not created by randomized controlled trial, although experimentation has been very important for both those companies. And in the book, I have half a chapter, probably less than half a chapter about randomized controlled trials. But the whole of the rest of the book is still about experimentation, it's still about trial and error, just on a much more informal basis.

So if we think about the basic ingredients, we're going back to those Peter Palchinsky principles -- it's lots of new ideas, the ideas need to be small enough that they're not dangerous, and they need to be selected one way or another.

So, the pluralism is important. We need organizations, organizational structures, that can tolerate pluralism. So, I'm not as worried as many people are about fragmentation in the aid industry. It can be pathological, but we could also harness it, we could turn fragmentation into pluralism. And I'll tell you that aid agencies are not the only actors in this space either.

So, the idea that development projects don't have to be like Coca-Cola. You know what Andy Warhol wrote about Coca-Cola, "all the Cokes are the same and all the Cokes are good". So we want all the development projects to be the same, and all the development projects to be good, but actually those two things are in conflict. We need to tolerate the fact that they will be different, we'll be experimenting.

And then, anything that increases the effectiveness of feedback will work. So, the Center for Global Development, I think, has been behind some very interesting pushes there. So there's the advanced market

commitments that I know you, Owen, were one of the champions of. And there what we're saying is 'we're putting donor money behind important drugs'. But the people who have to take responsibility for creating the drugs are pharmaceutical companies, and they won't get paid until the drugs are produced. In fact, it's more than that. Not only won't they get paid until drugs are produced, they won't get paid until somebody somewhere is willing to buy these drugs, albeit, at a subsidized price. And they will get paid in proportion to the number of doses that they sell.

So this is clearly a strong feedback move – and I think early signs are – I know the first vaccinations were given out just a few months ago, that it's been very effective. And we need more of that. Another example, Cash On Delivery Aid, another project from the Centre for Global Development. I'm not just being polite because I'm sitting in the Centre for Global Development, but obviously the whole idea of the Centre is to try new ideas and to insist on good evaluations. So they're very much in sympathy with the ideas behind the book. So, Cash On Delivery Aid, we will agree with the Department for Education in Kenya that they – we want certain outcomes from Kenyan schools, certain numbers of children to graduate with certain reading or mathematical abilities, say, and we will pay them when they deliver. And clearly, the Ministry for Education in Kenya is not the only actor, and they're not on the ground, they're not school teachers, but they're probably closer to the ground than a donor. So, we're pushing it down and we're letting them experiment, letting them figure out what works and we're incentivising it.

And a third example, a much grander example, a much crazier example in a way, and this is not a Center for Global Development project, Paul Romer's idea of charter cities. So, Romer's idea of charter cities, which many listeners to the Development Drums podcast will know about, is wouldn't it be great if governments set up cities where different rules of engagement applied, different infrastructure, and different regulations. And Romer also says, 'maybe we could get other governments to supervise those areas?'

Now, that of course is what's raised a lot of heckles. I don't see that as essential. I know Paul thinks it's important, but for me what's really interesting about the charter cities idea is it's an experiment on a very big scale, but not a catastrophic scale. These cities can fail without untold human misery. People could just not turn up, or they can succeed, and they're valuable because they allow governments to try something new without having to reform regulations everywhere in their economy.

And there are many, many other examples, but experimentation is vital. But it doesn't have to be a rigorous randomized controlled trial. It could be a much more informal process.

Owen Barder

What can we, as outsiders, do to create conditions in which development organizations can experiment in that way? I mean, the institutions we have evolved within the environment they're in. And on the whole they haven't evolved good mechanisms for learning because they haven't needed to, they have continued to get funding, from tax payers mainly, without doing the kind of experimentation that you're talking about. What is it that we can do, people who are listening to this podcast, either who are working in these organizations or who are working in the development space to try to create the conditions? Because it seems very difficult for organizations to make this leap into being more experimental – we talked about the global fund example earlier. There isn't much tolerance of failure; the media pick up the one failure and exaggerate it. What are the things that would make it a more conducive space and what do you think might happen if we don't do it?

Tim Harford

If we don't do it, more of the same, which is an industry which despite tremendous resources is not as effective as it could be and not as innovative it could be. And this is one of the most important problems in the world so we need to do as well as we possibly can. I'm not that interested in this general question of whether aid works in general, in abstract or not, much more interested in finding out which parts of it work and which parts of it don't.

So, I think one thing is – let's be positive, let's just look for improvements. So rather than this question of is it good or is it not good, let's just go straight to – cut the nub of it, the question we care about was how do we make it better? What are the specific things we could do to make it better? So that's one thing.

I think we owe to ourselves and to sort of proper discourse in this environment to take evidence seriously and not to be ideological and not to say 'well, these guys are the good guys, this particular economist or this particular project or this particular institution is on the side of the angels and then they should need unconditional backing' or vice versa, 'those guys are just idiots, they never get anything right.' We need to be interested in evidence and be supportive of evidence.

And I would like us to be tolerant of honest failures. I wish development organizations, in fact political institutions of all stripes would far more often say 'we did this, we thought it might work, we evaluated it. It did not work. We're going to stop doing it.'; And this got round of applause, but instead we talked about U-turns and wasted money. And that's downs to us, as voters for instance we don't tolerate that in our governments. And we have to, otherwise we get the governments we deserve.

Owen Barder

So, Janine Cooper asked on the Facebook page for Development Drums whether you have any suggestions for people who want to get funding from donors for experimental approaches. Are there any things that you think will make donors be willing to fund those kinds of experiments?

Tim Harford

Oh, gosh, so I've never applied for donor funding, so my advice is probably going to be very bad. But I think, to look at the history of randomized controlled trials or experiments more broadly in medicine and in development, but medicine there's more of a history, and just to demonstrate time and time again that good evaluations have tremendous scientific power. They tell us a lot. They also have tremendous rhetorical power, you can say to a donor 'look, if we do this and if it doesn't work we've learn something important and if it does work we really got incredible ammunition'. And the moment we've got just the same few randomized control trials being talked about because we haven't got many. And we need to change that.

So, that's the thing. Evidence has scientific value, but it also has rhetorical value. I wish we had more rhetorical value, but it can still be very powerful.

Owen Barder

I've been pretty struck actually how much money has been drawn into the big vertical health programs. And I suspect that's in part because those health programs are largely supporting interventions for which there are randomized controlled trials, particularly drugs. And they're able to say with much more clarity what it is that they're achieving than alternatives. For example, we don't have very much money being sucked into health systems because we haven't got very much experimental evidence about what kinds of intervention work in health systems.

So it seems to me weird that more of the development industry hasn't taken this up just in order to compete more aggressively with the funding that's being sucked into those small number of spaces where there is RCT evidence available.

Tim Harford

No, I think that's absolutely right.

Owen Barder

One issue that – we've been talking about the way that the development industry evolves, but there is a – can we widen that out to the question of what it is to develop? And there is a story that says that what it is to be an advanced economy and a successful society is to have successful problem solving institutions. You have functioning government, you have functioning market economy, functioning financial markets, functioning health system. So, do you think that there's a lesson in your book for how we go about creating the conditions in which those things emerge? Not necessarily as aid agencies but what governments and

citizens should do in their own countries to try to accelerate the development process? Is that a – too big of a stretch from what it is that you're saying?

Tim Harford

It's an issue that I look at briefly and the research side I describe I think is fascinating, but I don't think it offers any answers. So I look at research by César Hidalgo and Ricardo Hausmann. Ricardo's an economist at the Kennedy School of Government, César is a physicist at MIT, and they've worked together – it's fascinating work – on I suppose what you would call the structure of product space. What they're trying to discover is, well first of all, what kind of products are like other products? So, are apples like pears? And sometimes it seems obvious that apples are like pears, it may not be obvious, and the way they create that information is to look at exports and to say, if lots of countries export apples and the same list of countries also exports pears, therefore apples are like the pears. And it may be to do with climate, it may be to do with phytosanitary regulations, it may be to do with access to ports, or skilled agronomists, I mean there are lots of things it could be down to. And the most sophisticated the product the more sort of things are underpinning it.

So, they for various reasons to believe that there are – there's a huge variety of what they call capabilities that make it possible to produce certain kinds of products and services. We cannot observe these capabilities and anything – the kind of thing that we talk about institutions: anything from a particular detailed regulation of mobile phone licensing to the fact that an internet business needs a credit card network and functioning addresses and a non-corrupt post office. I mean there's so many different possible capabilities out there. So they're looking at these capabilities and are trying to understand how countries move across capability space and acquire new capabilities.

And there's an awkward message really for both sides of an old debate, the big push debate. Because what Hidalgo and Hausmann show is that you possibly do need big pushes sometimes to get an economy from a backwater in product space where they haven't got the right capabilities and there's nothing they can do, there's no sort of easy steps they could take to acquire the right capabilities, it needs some dramatic move; so that's an argument for industrial policy. But the sheer complexity of the product space and the complexities and the capabilities that underpin it that we don't really understand tell us why industrial policy is often disappointing. So I describe that work, but I think it leaves us with the challenge, but it's not a challenge that's easily solved.

Owen Barder

So Robbie Barkle put exactly that challenge to you in – on Facebook, which is, if these things are complex, if development is a complex system, does that mean that essentially government – the space for government policy and government intervention is narrowed because the effects of interventions are unpredictable in a complex environment?

Tim Harford

Oh no, not at all. I think that we just have to be adaptive when we take – when we make those interventions. We're not talking about a nuclear power station here or a fragile financial system, although there is a chapter in my book about those things. But in development space I think there's less of that fragility. These are very important problems. I don't think they're massively fragile problems. There are lots of things governments can try on a small scale and see if they work, whether it's a regulatory reform or a piece of infrastructure. Donors can do it, governments can do it, NGOs can do it. And the fact that the product space is complex simply means, well, you need a lot of experiments and a lot of feedback. But you can't move through a complex space.

Owen Barder

You don't have a problem of high risk of failure, or don't you have a problem that failure is a catastrophe.

Tim Harford

As long as the failures can be kept small, which I think in many – although people's lives are at stake, they are also at stake, say, when we do medical experiments. And we've learned – the information is so

important we need to produce it. So as long as the failures can be kept small and we can learn quickly and move on, that is how complex problems have always been solved.

Owen Barder

So I'd like to change gear just at the end to – when I read your book I was – I rather envied your ability to weave together these different stories, these different examples into a compelling overall narrative. I wondered as a matter of – how you go about doing that. Do you start off by collecting all the stories and then think 'what's my overall narrative?' Or do you start with – how do you find the stories? Do you have a card index of everything you read, every newspaper article, you write a little note? How do you bring together this big picture story with all these compelling examples that bring it to life?

Tim Harford

Well you're very kind. And every book is different and this book was very much a – appropriately enough, an exercise trial and error. So trying to figure out what the book was actually about. It was initially 'how could economics save the world?' and gradually became 'well, who cares whether the solutions come from economics or not? Let's just talk about saving the world.' And then rather than a laundry list of possible policy problems, which is not very interesting and not very plausible either, it became a realization as I started looking at more and more of these problems, well actually trial and error comes up again and again, experimentation comes up again and again. And then it's like you buy a new car and you start seeing lots of new car around. So you start discovering ideas and examples of experimentation that you weren't aware of.

So just a couple of examples, I'm big fan of Ben Goldacre's book *Bad Science*. It taught me a lot, it gave me a lot of context on randomized controlled trials. Ben Goldacre mentioned this guy Archie Cochrane who inspired the creation of the Cochrane collaboration. In passing. He said 'oh he's a really cool guy'. And I said, well, I'll read Archie Cochrane's autobiography. And he's not just a cool guy, he's completely amazing; he's just – he's a hero. He did amazing things and saved lot of lives and put a lot of noses out of joint. And – but you can read a biography or you read something and actually it turns doesn't – it's not very interesting, it doesn't particularly add anything to your thesis.

Or just another example, I was interested in writing about the insurance crisis in the British insurance industry in the late 1980s, because it's like a mini financial crisis. That crisis was triggered by a terrible accident called the Piper Alpha disaster; huge oil rig in the North Sea blew up and over 150 men died and they triggered this insurance accident. So I thought well, it would be – I should write about this disaster briefly and explain what happened and then I'll talk about the insurance business. My sister is in safety engineering as it happens. So I got a bunch of books about, these safety engineering books which contained descriptions of Piper Alpha and I started to read these books and discover, actually they're describing tightly coupled complex systems. They're describing systems that are like banking systems and one book in particular, *Normal Accidents* by Charles Perrow, the epilogue is all about, well, how do we use these insights from safety engineering in the financial system? It's written in the late 1990s. So that was just a serendipitous discovery that you understand what happens to a nuclear power station that tells you something about Wall Street. So in short you have to read a lot. But that's okay because reading a lot is fun.

Owen Barder

You've been listening to *Development Drums* with me, Owen Barder, and my guest has been Tim Harford who's new book is called *Adapt: Why Success Always Starts With Failure*. Tim, thanks for being on *Development Drums*.

Tim Harford

Thank you very much.