*Annals of Glaciology*

**Article**

# Open discussion at the IGS symposium on 'The edges of glaciology', 7 July 2023

Andrew C. Fowler

MACSI, University of Limerick, Limerick, Ireland

**Cite this article:** Fowler AC (2025). Open discussion at the IGS symposium on 'The edges of glaciology', 7 July 2023. *Annals of Glaciology* **66**, e3, 1–9. https://doi.org/10.1017/aog.2024.42

Received: 19 September 2024
Revised: 6 November 2024
Accepted: 7 November 2024

**Keywords:**
applied glaciology; glacier flow; ice dynamics

**Corresponding author:**
Andrew C. Fowler;
Email: Andrew.Fowler@ul.ie

## Abstract

There follows the open discussion which took place at the IGS symposium on 'The Edges of Glaciology', in July 2023. The discussion was curated by Doug Benn. The time of speaking in minutes and seconds into the Panopto recording is given in bold figures. The recording itself is provided as electronic supplementary material. It has been transcribed and edited by Andrew Fowler, with much (and much-needed) assistance from the participants. Footnotes (mostly references) are editorial intrusions.

**1.38** Doug Benn, University of St. Andrew's: The idea of this session, Andrew had the idea of reviving the old general discussion that used to be a feature of IGS conferences a long time ago, and so really, this is going to be the opportunity to air and talk about some of the things that we've shared over the course of this conference. Now, I've taken the liberty of identifying three themes that provide a starting point for discussion, and this is purely a personal, subjective choice; but hopefully, it provides an opening for most people to discuss the issues that interest them. And the first of these themes concerns ice dynamics, and in particular the linkage between the detailed process studies on the properties of ice and the rheology of ice, the properties of the glacier bed and processes that control slip and deformation of the glacier bed and how can we make the link between these detailed studies, what's been found there, and the larger scale glacier dynamics, the kind of rules we need to prescribe the relationship between velocity and friction in ice-sheet models. So, just thinking about that scale link.

The second thing concerns self-organisation in glacier systems. This has popped up in a number of talks; I was obviously talking about this in the calving talk, but it's also a feature of drumlins, that we looked at yesterday, roll waves on ice shelves and the velocity structure, you know, the fact that glaciers go from one flow mode to another, and the transition between states that the glacier is organising itself into under different conditions.

The third one concerns collecting the information we need on glacier behaviour. So there's been a lot of talks that have been pushing the frontiers of what we can tell from remote-sensing techniques, and the insight that that gives us into how particular systems are behaving. And so I think it would be also interesting to talk about the frontiers in remote-sensing techniques: what we'd like to know, what is the next stage, what is the next generation of remote-sensing techniques.

Okay, so to begin the first one then, processes of ice deformation and processes at the glacier bed. Would anyone like to begin?

**5.00** Dave Prior, University of Otago: I want to just make some statements from the point of view of an experimentalist about ice, the flow laws that describe the deformation or the creep of ice, which I think have an impact in a lot of areas where people are trying to model the behaviour of glaciers, ice sheets or whatever.

And the first statement, which might be a bit controversial, is that what people commonly use as the Glen law, and by that I mean a relationship between stress and strain on which people put an exponent of 3, and it has a temperature dependence, but it doesn't have any other prescribed dependence, is a kind of misreading of the experimental data and that flow law in that form doesn't really have any useful application, apart from to ice at exactly the same conditions of grain size and stress which will give you $n = 3$.

And if I attack two aspects of that: Glen knew this, and I think Glen would be a little bit horrified by the way it's used at the moment. So, in his first paper, which is 1952,[1] he said $n = 4$. In his second paper of '53,[2] he said, he then discovered that there's a way of doing a

---

[1] Glen JW (1952) Experiments on the deformation of ice. *Journal of Glaciology* **2**(12), 111–114.
[2] Glen JW (1953) Rate of flow of polycrystalline ice. *Nature* **172**(4381), 721–722.

nice experiment where you go to what they call secondary creep which is at very low strain, and at that you get from his experiments a measurable $n$-value of 3.2 I think it was, and in that same paper he says if you go to higher strain, $n$ very rapidly becomes 4. In his big 1955 paper,[3] he says essentially the same thing. In his 1957 and '58 review papers,[4] he says that $n$ varies between 2 and 4, which is something we know since then; and then it's other people, and Paul Bons has done a really good job of unpicking the route to the $n = 3$ flow law[5] and I won't say the names of the early people, but other people just said, oh, we'll approximate this as $n = 3$ and it's kind of stuck there.

So if I can dial forward now to what we understand from experiments: whenever you push an experiment to high strain and what I would call a steady state, you get a simple relationship which is an $n = 4$ relationship, and there is a flow law for that that Bill Durham published,[6] a flow law in 1983 which is based on steady state experiments, which has an $n = 4$ flow law with a temperature dependence but no other dependence on grain size or anything like that.

And we understand that there are grain size evolutions and evolutions in fabric, which mean that at steady state, you can use that to kind of simplify. The grain size is one we understand better. Grain size when it evolves basically depends on the stress, so if you have a grain size sensitivity (the stress is dependent on the grain size and the grain size is dependent on the stress), you can cut grain size out of the equations. So if you're dealing with a model which has a long time scale within it, so that you can think of the ice as being in the steady state in that model, an $n = 4$ simple flow law is probably quite appropriate. If you're dealing in a model where you're not in steady state, so the ice is undergoing some kind of transitional behaviour (a really good example that definitely fits that would be flexure, for example), you need a more complicated flow law like we've heard in some papers, like the Goldsby–Kohlstedt flow law,[7] which involves grain sizes and so on, potentially.

**9.01** Felix Ng, University of Sheffield: I just wanted to add a little bit to the periphery of David's outline. It is that ice-sheet modelling hasn't really gone into using anisotropic rheology. So that's one clear frontier that glaciology ought to explore.

**9.30** Brent Minchew, M. I. T.: Yeah, so you know we can obviously learn a lot from experiments; but then when we're thinking about the ice sheet, there's a lot more complications that come into the natural environment, you know, air bubbles and impurities and all these things. So, I think we're getting to the point where we have better and better data from the field. Obviously we use a lot of remote sensing for those kinds of things. And to start thinking, I guess as a field, it would be nice to start thinking more about how do we span these scales from the laboratory scale all the way up to the ice-sheet scale, and make sure that we're doing experiments within the right parameter space that applies to the ice sheet, as well as taking these ideas and starting to test them in ice that is at sufficiently high strains, has damage, has air bubbles, has all kinds of other complicating factors that can play into affecting the rheological mechanisms that go into ice flow laws and other bits that we have; so, better calibrating parameters for whatever flow law we use for the natural system.

**10.43** Luke Zoet, University of Wisconsin: I think that I can not necessarily be more general than that relative to ice flow; but you know we can do things at a small scale in these processes that add such complexity, that you never really want to put it in a full-scale ice-sheet model. It would be too complicated with those in there. And I think that, you know what, in some ways what's needed is more intermediate scales so we can understand the processes on the smaller scale, and in great detail, and understand them. But we need the intermediate-scale modelling that takes into account how to combine all these aspects which Brent's talked about; we know the fundamental processes, but we've got to add the complexity that we know that's in the field, not at a scale that's in an ice-sheet model; and then understand how to parameterise in general something that's true to the actual physics, but in a way that can be used in a larger model that has enough complexity that you get it right, but not so much complexity that you create a huge burden for the people that are trying to model things at a larger scale, in a way where they just say, well, you know, it's too complex, so I'm not going to do it, right?

I think that what has happened is that people that work on my scale said, this is what's happening, and people who work on a larger scale say, we can't put all that in the model. So there's been this sort of separation of the two. We need people in the middle that can take things that are realistic physics, but then put them in a way that can be used for the larger scale.

**12.23** Bernd Kulessa, Swansea University: I was also actually going to say something very similar, because I spoke to some folks here about some groundwater modelling in the 1980s and early 1990s; there was a huge development in groundwater flow laws and so forth, and they got to the point where it got so complex with so many free parameters that they actually realised this is completely unusable now. And there was a massive landmark session or workshop where they basically got together and tried to figure out, how do we pull back from this and move to something that's usable, and captures the essence of what we're actually trying to model here. And so we see with ice-sheet modelling, we're nowhere near that complexity now. Maybe we can learn a little bit from that, like Luke says: basically, as we are developing things, let's not get too obsessed with having all these detailed processes and trying to capture them in a model, but rather in the essence.

**13.19** Andrew Fowler, University of Limerick: I would, just on that, make a comment about the process of modelling. I'm thinking not so much of what ice does, which from

---

[3] Glen JW (1955) The creep of polycrystalline ice. *Proceedings of the Royal Society of London* **228**(1175), 519–538.

[4] Actually, both 1958: Glen JW (1958) The mechanical properties of ice. 1. The plastic properties of ice. *Advances in Physics* **7**(26), 254–265; Glen JW (1958) The flow law of ice. A discussion of the assumptions made in glacier theory, their experimental foundations and consequences. *International Association of Hydrological Sciences Publication* **47**, 171–183.

[5] This was a personal communication.

[6] Durham WB, Heard HC and Kirby SH (1983) Experimental deformation of polycrystalline $H_2O$ ice at high pressure and low temperature: preliminary results. *Journal of Geophysical Research* **88**(S01), B377–B392.

[7] Goldsby DL and Kohlstedt DL (2001) Superplastic deformation of ice: experimental observations. *Journal of Geophysical Research: Solid Earth* **106**(B6), 11017–11030.

my point of view is relatively simple, but the other part of this which is at the bed, where I don't have a very good sense, as somebody who tries to model things, of what the picture is. You know, you have water, for example, at the bed if it's sliding, there's water in the till, there are streams, there are cavities, there's all these kinds of things. And I don't really have in my head a very good idea as to what the, if there is a correct picture, what it is. And as you say, there are a lot of complexities, and you can add more and more things. And then, then you have to ask the question, stop putting the kitchen sink in. How do you decide whether you've got the right ingredients in your sink?

**14.34** Ian Hewitt, University of Oxford: It seems to me that one of the ways that the modelling community is developing a lot now is to fit both viscosity or flow law curves, and sliding curves, things that vary in space and you allow them to vary in space in a way that allows you to fit some observations, usually of surface velocity. And I guess the hub of the idea there is that that is sweeping under the rug all sorts of other dependence those coefficients might include, like grain size and temperature and water content, damage …, all sorts of stuff to do with cavities, till beds. And if the goal is to reproduce current-day observations, then that seems like a reasonable approach.

But of course if you then use that to evolve things through time, you need to be aware of whether those things which are fitted are actually expected to be constant. And I guess one of the things that I think is not clear to me is, what are the most important time dependences in those quantities we are trying to include? So whether that's evolution in temperature or evolution in fabric, for example, that's not clear to me. And obviously that's going to depend on what time scale you're trying to evolve in.

So if you're looking at the next year, say, then some things are going to stay constant; the ice geometry is going to stay relatively constant on that short time. But that's obviously very different if you're interested in glacial cycles. But I think that the approach of allowing parameters to vary spatially is increasingly what's used. And I think it's very important then to think about what it is that you're allowing those parameters to be.

**16.45** Paco Navarro, Universidad Politécnica de Madrid: There is another problem with the parameters, that is that for instance we have done many experiments where we were varying the viscosity on one side and then the basal condition on the other, and then you try to do maps to minimise the error between observations and model predictions; and then ideally do the spread to see some map minimum there, so that that will give you the proper value of both. But what you usually find in practice is some kind of value that means that there is an infinite set of relationships between two different parameters which provide exactly the same error and then the same solution. So this is a problem which is always present. And then it's important to distinguish between the processes, and this is another difficulty.

**17.46** Minchew: So just to push this, I guess, a little bit further, ice of course flows like a viscous fluid over certain time scales, but then it also fractures. And so, understanding this transition from this ductile to brittle régime is perhaps a new frontier in rheology that we haven't really started to explore, I think, in great detail. I think often in our models we would treat ice as though it's a fundamentally different material, depending on whether or not it's flowing as a viscous fluid, or it's fracturing as a solid. But somewhere in there we need to start building up an understanding of the conditions under which this transition starts to occur, and then think about, again, the detailed processes.

One of the things I mentioned, Doug and I talked about this quite a bit, I mentioned it to a few other people, I don't think we have a great understanding of why ice fractures at all. And you know, clearly it does. And there's kind of a simple and naïve answer that says: if you hit anything hard enough, fast enough, it's going to break. But we often see fractures forming in places and developing, such as rifts, where the time scale of build-up of stresses is much, much longer than the relaxation time. So we should be solving in the viscous régime, and yet the material does fracture. We see this from observations. So understanding that physical phenomenon, and how we see that transition, it's going to be important. I think, even if we're doing these relatively simple intermediate-scale parameterisations, we really need to capture that properly within our models. I think it's going to be a huge step forward.

**19.29** Benn: Yeah, I think that point leads very neatly on to the second theme that I was proposing, the tendency of glacial systems to organise themselves around particular states, and how robust are those steady states, how sensitive are they to perturbations and you know, what is the magnitude of perturbations they were subjected to, and what causes transitions to other states? And I do think this principle has the potential to help us bridge the gap between the detailed process level and the global glacier behaviour of the system. Anyone have any thoughts on that one?

[*Silence.*]

Okay. Well I will continue and then hopefully I'll poke somebody with a stick at some point to get a response. We were talking, for example, about ice shelves and the collapse of an ice shelf or where it's going from the state where it's been sitting at a particular position for decades, or as long as we've been looking. And then it collapses. And there has been a tendency in research to focus on what is the trigger of that collapse. So what event was it, a big melt year or melt ponds forming or wind stress or wave stress or whatever; to focus on what it was that hit that system, that made that thing happen.

But I think the important point is to remember that that system had to reach that state, that point of sensitivity, before it was susceptible to that particular trigger. And so it's important to bear in mind the clear distinction between sensitivity of the system and the magnitude of the trigger. And another example, Ian showed this earlier on, is from the surging data. During quiescence, those glaciers are insensitive to the annual meltwater pulse in the summer time because water isn't getting to the glacier bed. But through gradual changes, the surface begins to become crevassed and then it becomes sensitive to that forcing and *poof*! The surge takes off.

**22.29** Fowler: I make a small comment because it is something that I notice a little, as you mentioned triggers, and in particular in the context of surging as an example. Some people will have an idea that something dramatic happens and so there must be an immediate cause. My own view is it's just a longer time scale thing. So for me, a surge is an oscillation, just like a pendulum going backwards and forwards. And it's a little bit as if you took a photograph of a pendulum at its extreme angle and then noticed it was going to reverse and go the other way. And then you might say, what caused that? But it's part of a large-scale phenomenon, and I think there is a perspective there to be drawn.

I mean, actually, talking of drumlins, as you do, you have within the whole drumlin gang of people, there is a, let us say, a tussle going on between those who focus on an individual drumlin and its interior and so on, and those, which would include me, who are less interested in the individual and more interested in global pattern.

**24.12** Minchew: So, picking up on that thread, related to that, of course, it's a good philosophy that Andrew articulates and it's useful to think about, especially for those of us who make observations of the modern system, recognising that really what we're doing is using often high frequency variations, high frequency being defined as within the observational record. My usual joke is that palaeo to me is everything that happened before satellites were flying. And so we're kind of using these pieces to try and construct an understanding of the physics of the system that play out over much longer time scales.

So we start to think about this, and you can think about this mathematically in terms of transfer functions or dispersion relations, whatever it is that you want to do. We need to understand that high frequency variations offer some window into the physical systems that we're interested in, physical processes that we're interested in, but that those processes don't necessarily play out in the same way as we get up to longer and longer time scales. So we work through these bits of data, and as we get more observations, we keep in mind that sometimes the most interesting pieces that we're seeing really are just oscillations over longer time scales. And so, we need to construct this picture of things and resist the temptation to become too myopic, focussing in on timescales that are within our observational record.

**25.46** Hewitt: I was going to make a similar comment, but maybe more from a modelling perspective: that at least when you construct a model, you should sometimes think about forcing. And it's become now something that I started to think more about, having stochastic forcing. And there's this issue about what we consider a model as trying to represent, and what we consider as noise. And I think sometimes it's quite hard to know what you should treat as the thing you are trying to predict with the model, and what you should consider as noisy. And I think this is particularly the case for ice sheets, when we're thinking about longer time scale questions. For example, about stability; what you should think of as the forcing. Because most of our observations are actually of extremely short time scale processes compared to the time scale on which the ice sheet evolves.

And so of course there's a tendency to look at the details of what's going on those time scales. But for a lot of models, that really I think is best treated as noise, and therefore you should not expect the model to be able to describe what happens when a tabular iceberg breaks off, for example, because that's part of the noise and the response should therefore be part of the noise and not part of what the model was trying to describe. But that depends very much on what you're trying to model.

And I guess this is known completely in climate dynamics, that people provide data for climate, that the weather is what we experience day to day and we get huge changes in temperature for example, that we try and explain as due to certain forcing. But that's very different from what you talk about if you're trying to describe climate change, what the forcing is that does that. And I don't think that this is always recognised in the context of modelling certain aspects of climate.

**27.44** Olga Sergienko, Princeton: Just following Ian's question about the climate forcing. It seems like at this conference, this group of people in particular is focussed on ice sheets isolated in the climate system, but unfortunately they cannot be completely isolated, no matter how much we want to do that, regardless of the time scales we are looking at. And the simple reason is that ice sheets are fully integrated into the Earth system climate. When the climate changes, the ice sheets change in response, due to your 'forcing'.

And it seems to me there is very little effort (maybe we are not at that stage yet) where we can try to think what the feedback orders of magnitude actually might be and how that affects the right time scale that you learn through various applications, and the problem becomes much more complex; but don't forget about Ian's question, the immediate question, what is it that we are trying to do?

**29.03** Navarro: In any case, this is the natural evolution of the experimental science. And if something happens, you don't bother about it because it happened. So suddenly there is an ice shelf disintegration for instance. And of course, you look for the immediate cause, but it is because the models you were considering were not considering this, even this possibility. And then you refine your models to do things or you see a really faster process that your current model does not predict. And then you start thinking about why it is, for instance, about the rôle of the ocean and so on, and you include new things in the model.

So I mean, I understand your point that you don't have to look for immediate causes. But in any case, this is not an evolutional thing that you are just improving models as some unexplained things by your current models; I don't have an answer for that.

**30.08** Kulessa: So I was thinking a lot about the ice shelf stability and the longer term forcing, and you know, people have made these great maps now of which parts of an ice shelf really matter for buttressing. And then Hilmar showed a few years ago[8] that as soon as you change that a little bit, then the glaciers behind flowing into the

---

[8] Gudmundsson GH, Paolo FS, Adusumilli S and Fricker HA (2019) Instantaneous Antarctic ice sheet mass loss driven by thinning ice shelves. *Geophysical Research Letters* **46**(23), 13,903–13,909.

ice shelf, the velocity basically changes immediately. So there seems to be this really intimate connection between the two effectively happening. And so I think there's a great opportunity now to actually monitor that behaviour over time and try and understand the physics of it.

And I think one of the things we need to be careful about as a community is that there seems to be a tendency to blame everything on the ocean and ignore everything else. A lot of people say, if you monitor the ocean on its own, then if we do that, then we can. But you know, if you have a glacier that's frozen to the bed, then on the extreme you can change anything you like in the ocean, nothing's going to happen. And it's the same with pinning points, ice shelves are actually very often very complicated. So there's a great opportunity here. But I think we need to keep an open mind as to all the different triggers that are important and active. And that's a self-organising system ultimately.

**31.31** Benn: Yeah. Olga, you made very well the point earlier on in the week that binary division into stability and instability is not meaningful. And in terms of system sensitivity to the particular range of the forcing, it's a much more useful and flexible way of thinking about things.

[*Pause.*]

Okay. Shall we move on to the final theme that I suggested, which concerns remote sensing? And Bernd, you were touching on that in your last remarks. And in terms of where are the frontiers? What would we like to be able to measure? What platforms are coming down the line? What are the next steps in remote sensing? Let me see. What other information can we dream of capturing?

**32.45** Minchew: So I'm sure that I've talked most people's ear off about the kind of things that we're developing, which we can get to in a little bit in terms of platform capabilities for remote sensing. But what I would say is that the next frontier in terms of surface observations, the main result of that is getting down to higher resolution over space and time, and higher signal to noise ratio strain rate fields, so that we can start to ask deeper questions about rheology. We can start to better understand calving processes and so forth, but using the strain rate fields, and then trying to gain some insight from those into the stress fields themselves, and simplification of stresses and response of the ice deformation rates and so forth to various stress positions.

And I think that there's a new generation of satellites coming online which NISAR is flying in six months or so; they'll give us data in about a year that we can start to use; that would be a big step forward. And in those processes, and then there's a lot of things coming up in terms of using high altitude, long endurance platforms like drones and things of this nature. So we're sure that everybody probably knows this by now: we're working on a solar-powered drone, for example, that should be able to fly for about four months at a time over the ice sheets, collecting InSAR observations at spatial and particular temporal resolutions like we've never had before.

And those platforms are brand new because the technologies that we needed in order to build these platforms are just now coming on line; those technologies will continue to improve. And so we'll be able to push out beyond surface observations and hopefully decadal time scales, and start putting things like penetrating radars on these platforms and start to get an understanding of the vertical structure of velocity and basal processes, things of this nature. So that's the kind of 5 to 10 to 20 year outlook, at least from the perspective of things that I do.

[*Pause.*]

[*Benn: There's a lot of remote sensers in the room, let's hear from you.*]

[*Pause.*]

**35.28** Ng: I think perhaps we could also broaden the observational frontier to go to geophysics that Brent alluded to, and different ways of probing deep bed processes and strains.

**35.46** Hewitt: I'm definitely not a remote sensor, or senser. One of the things that I can see happening, which I think probably would be a good thing, would be for more integration of data and collection and models to the extent that it comes in a sort of streamlined way. My impression is that we still at the moment think of data as separate from models, and then the data might be used to pick a model. And I think it's partly because the amount of data that is produced is ridiculously large.

I think it would be good to move towards another way where you integrate the way we collect the data which fit in a model. And I think this happens a bit in weather forecasting. I don't know very much about it, but you often get reanalysis products which are basically fitted models. And that doesn't seem to have happened at all in the ice-sheet modelling community. I can imagine that would be something that would be possible in a lot of ways of observing. And so I don't know whether that's something that we could be getting into, which is basically coming from some time-dependent data assimilation, which I think is something which is increasingly going to happen.

**37.09** Prior: So one of the comments I'm making, this is more about surface ice, and maybe airborne geophysics and things like that. One of the things I feel is we need a lot more ground truthing of the inferences that come out from those, particularly when you're using the data for proxies for, you know, fabric is the one I'm particularly interested in. And there are very few places where people have done that kind of ground truthing where they actually have direct measurements from the ice, together with some kind of proxy. And often the places where the ground truthing is done is in places where we happen to have ice cores, which are domes and ice divides and so on, which are not necessarily the same as some of the places we have most interest in from an ice dynamic point of view. I can understand the problem, it's expensive for all of those things together, but I think we need a lot more of that.

**38.16** Kulessa: So that's a good point, I was thinking about this, and also what our group was doing earlier with the effective stress, trying to estimate the effective stress from the seismics; and then in many fields there's a disconnect between what we measure and what the ice-sheet models actually need. And you talk to modellers, and I show them my geophysical results, and they start laughing and say, what do you want me to do with this? And that actually is now, you know, an early career drop. And some people like Felicity McCormack, for example,[9] like explicitly embracing the challenge of integrating data into the ice-sheet models. And I think, you know, to me, that's actually one of the most exciting and refreshing movements under way at the moment. And I think if all of us embrace that sort of undertaking and really try and help with it, that would be a fantastic outcome.

So that was one comment. And the second one is, as we are doing this, another thing that's really struck me over the years is that this notion that as we are losing ice mass, that the crust underneath rebounds and can actually potentially restabilise the glaciers or ice sheets, as the record has shown. And at the same time, we've now seen that if you've got fast enough advance and retreat rates and the amount of groundwater you put into the crust and the amount of groundwater that comes back out again actually matters a lot, because there's a lot of coupled geothermal heat flux that comes with that as well. And so I think that that to me is another big new frontier we could think about. It's not even that surprising because the palaeo community, and you probably know this, knows that groundwater plays a fundamental rôle in all the palaeo-ice-sheet behaviour. But then I'm a modern glaciologist and we look at a modern ice sheet and we tend to just ignore that. And that's a real dichotomy there.

**40.08** Sergienko: Just a few statements. I'd like to start with a very obvious one, glaciology is an observational field. Another statement is that I do greatly appreciate what all this remote sensing has measured and observed, but for ice-sheet models it's not exactly the kind of data that is helpful and useful. So thank you, it's really great, but it's not directly useful in the model. It's what Ian said, effectively.

I would just like to mention that in 1993 Doug MacAyeal[10] proposed the control method for optimisation for different parameters, that's where we are at the moment.

So in that regard as a modeller, my personal preference would be to find ways to collect different kinds of ice flow data. Ideally, for the ocean and the ice sheet, taking a look at the bed and put it back very, very quickly in order not to perturb it. But on a more serious note, it's what's happening in the ice shelf cavities, we have no clue whatsoever.

When oceanographers come to me and say, 'Look, how does it look to you?', I cannot say anything, because we have no clue about how circulation happens, about the ice shelf interface. Same with the ice shelf bed, same with the interior of the ice sheet itself.

So, yes, we can invert a hell of a lot, but the system is so unconstrained that, what Paco was alluding to earlier, one can have different combinations of parameters that produce exactly the same result at the surface. So, point is, we need to think a little bit harder how to get all that information directly or indirectly that yields more knowledge about what's happening at the base, shall we call it? I think at the interface with the bedrock, or sediments or the ocean.

**42.26** Minchew: So just to tie some of these comments together. One is, I call this making it worse, is that we tend to observe things that can't be modelled, and we model things we can't observe. And in bridging that gap, this is kind of one of the great challenges for both sides in terms of the developments, I think, that are coming online. Just to broaden the things I was talking about before, there's a big shift in terms of observational techniques for cheaper instruments that still give us relatively good data.

And what that's going to do, of course, is just massively increase the volume of data that we have available to us beyond the point that we're going to be able to look at data and start to understand it in its own right. So this integration with models is going to become, I think, a necessary element, so that we can actually handle and consume the insight from the observations that we have, as well as pushing models forward, at least within climate dynamics. In this space there's a lot of development that's going on, where people are kind of rebuilding climate models from scratch, with the understanding that they're going to do it to assimilate data and allow data to drive forward.

So one of the big things that I'm aware of that's going on is this project known as CliMA, which is headed at Caltech and M. I. T., which you might know about. But, you know, they're building on this philosophy. So it's something that I personally embrace and I think a lot of people do too. It's really important what's going on, where they're developing this entire model from scratch, using modern machine learning methods, automatic differentiation and this kind of thing, to be computationally efficient. But they're doing it from this whole framework that says that, well, the observations that we have, good observations that we have, tend to be of high frequency variations because we only started collecting observations relatively recently; and we can use that and assimilate them into our models and start building out towards lower and lower frequencies, and longer and longer time scales as we start to develop these pieces.

And so it's a combination of both learning physics from data within the model, as well as pushing the model space forward and pushing out to try to extrapolate to lower and lower frequencies, and assimilate that. Those methods are being developed now in other fields and hopefully we can start to adopt them, bring them in, so that again we can at the very least start to deal with the volume of data that we're going to be faced with in the next 5 or 10 years.

---

[9] See, for example, McCormack FS, Warner RC, Seroussi H, Dow CF, Roberts JL and Treverrow A (2022) Modeling the deformation regime of Thwaites Glacier, West Antarctica, using a simple flow relation for ice anisotropy (ESTAR). *Journal of Geophysical Research: Earth Surface* **127**(3), e2021JF006332.

[10] MacAyeal DR (1993) A tutorial on the use of control methods in ice-sheet modeling. *Journal of Glaciology* **39**(131), 91–98.

**45.01** Zoet: You know, with respect to questions that people are asking, observations …, I think, as we've sort of gone in tack, people have been creating observations, collecting observations on bigger and bigger scales, right? They try to map more of the area. Whereas I think that an area that could help is what Ian was talking about. If you could collect time-dependent data at one spot for a while, you can help to understand some of these processes better, which will help going forward.

And to do that though, I think that we need to look outside of glaciology at the tools that we all just rank, that we all just are used to using all the time, and make new kinds of observations. I mean, other fields have tools that we don't even think about. Come in to glaciology, and these are the tools we're going to use to characterise everything.

Maybe that doesn't have to be the case. Maybe we can use new types of data in new ways that are innovative, to try to understand these processes in a time-dependent way, to give us some baseline so that we can understand what's going on. So rather than maybe expanding more and more in space, focussing in some area of time – I'm not saying anything expensive – but in addition to the new tools that help fill in some of these gaps, between what's going on and how is it changing in space and time.

**46.35** Navarro: I would like also just to remind you, because Dustin Schroeder is not here, that probably the largest observational gap that there is at present is for Antarctica, and is the lack of ice thickness measurements all over the continent, because of this lack of satellites recording this.

So he pointed out a good example of that, Mars. You have much more information than we have on Earth. So of course, we know the problem with the radio communication regulations that are preventing the use of certain frequencies for these satellites, also the ionosphere problems have gone. But then there's some other things to be explored such as these stratospheric flights and so on. So I would like to just remind these guys who had it recorded because this is a clear area.

So in fact, there are international projects now, for instance, in SCAR there is the RINGS working group, which is trying to work in an international effort, a really expensive one, just to try to collect ice thickness information all around the periphery of Antarctica.

They have made really huge steps. And then they are going by ships from gravity because, of course, you have ice, sometimes you have water, you have the ice shelves, and so on. So you need gravity methods. They do need the airborne GPR for the bathymetry, so it is a really positive thing, because of this lack of satellite orbiting and taking GPR data.

**48.21** Benn: Just one slightly different thing I'd like to throw in to the mix, and this is something that really struck me with your talk, Luke, this morning, and that is the importance of going back and re-examining the assumptions that have been made about historic data.

And I remember very clearly when that stuff first came out from the ice streams in the 1980s and the interpretation of the big, thick deforming layer. And big thick deforming layers were flavour of the month that were proclaimed as a new paradigm by Geoffrey Boulton at this time.[11] And these ideas become entrenched within the community, and I think there are probably far more of those lurking around in glaciology than we usually acknowledge.

These things have become entrenched in our minds, of what things are and what things mean. And so I think also using the tools that we already have in a more critical and systematic way can help us re-evaluate what's going on in systems.

**49.36** Fowler: I'd like to actually ask Luke a question about this because, you know, being a theoretician, people say this is how things are, and I tend to say, okay, that's how things are. And then, you have this concept of deforming till, and so on, and then you do all these experiments.

And so if there is deformation within the till, it's a very, very thin layer. And so now I actually don't know what has happened, because my concept is that there is actually field evidence for lots of churned up stuff. But the laboratory picture is sort of, that doesn't happen. So I just actually don't know. I'm quite happy for somebody to tell me this is what happens, and then I'll believe it. I really don't understand how I should be thinking about this clearly.

[*Pause.*]

That's a question. [*Laughter.*]

**50.54** Zoet: You know, I think my viewpoint on the topic is that there's all these really innovative studies that people have done with seismics to look at the depth of the deforming zone. There's all this fantastic work in glacial geology, glacial geomorphology, I think probably ill-advisedly ignored a lot by this community, that tells us a lot about the bed, where you can see some areas where there are thin zones of deformation. In other areas, there are thick zones of deformation.

I think what happens is, we say this is a deforming zone, but what does that really mean, you know, in terms of how much is deforming versus what's actually facilitating glacial motion?

I think in a lot of instances, the actively rapidly deforming zone is relatively thin, but in some instances you can have small strains that extend to greater depths. And here's the complication of that. If you have a small strain that extends to greater depth beneath a glacier for a while, those small strains accumulate, and you only have to get to strains of perhaps ten in the glacial sediment to make it look like a very developed till. And so while 99% of your motion occurs in a thin zone at the bottom, you know, the bottom metre of it deforms a little bit for a long period of time.

---

[11] Boulton GS (1986) A paradigm shift in glaciology? *Nature* **322** (6074), 18.

And if you go and look at it afterwards, or you look at a seismic wave through it, it's going to look the exact same for all intents and purposes. So there's a sort of confusion between what probably is really important, it is that zone near the top where most of the deformation is happening. But you can match those signatures at depth through long periods of time in glacial processes.

That's like, you know, I think Brent was talking about this, or Ian, which is that we see things at short times because that's what we study in the lab. But we need to think about what that means when we extrapolate to those longer time scales. And that's why I think going and looking at the drumlin fields and looking at these palaeo records are good because those are integrating these processes for a long period of time.

And, you know, these glacial geologists have been out there digging a whole lot of holes in this stuff for a long period of time. And sometimes they have pretty good ideas that you know, me and you have just passed over, because we think more about the mechanics. Glad you brought that up.

**53.30** Benn: Now then: just after twenty past the hour, and we should move gracefully towards wrapping it up. Just before we finish, does anybody have anything that they feel that we haven't talked about? Any points that are important that cropped up in the course of this meeting, that haven't been raised so far in the discussion?

**54.02** Fowler: This is actually just a follow up comment to something that Ian said earlier about experiments and scales and things.

The comment I wanted to make was just to emphasise that point that the model you build depends on the question you ask. And the question you ask depends to a large extent on the time scale and also the space scale at which you observe whatever it is you're observing.

So if you're interested in, for example, the deformation of till, you can ask the question, how does an individual grain get round another individual grain, the mechanics of that scale. Or you can look at the larger scale as Luke was just saying. You can look at larger timescales where, on a short time scale, you might think that deformation is in a thin layer, but after a long time, it doesn't look like that, perhaps.

I think that's just a general point, that it's often not really taken into account when people have disagreements about what they're doing, they're not really disagreeing. They're just on different floors of the building.

**55.38** Benn: Yeah, they're holding onto different parts of the elephant, the leg or the tail or the trunk. We need joined-up elephants.

**55.52** Ng: Just to add a little ingredient to the wrap-up discussion. Just in my mind, I'm just wondering from this meeting or from where we stand, in the field at the moment, we've had various edges of glaciology, including those discussed in the meeting. Do we have a vision of where the new edges of glaciology are?

**56.15** Geoff Evatt, University of Manchester: The answer's obvious.[12] We've had a wonderful week of doing stuff. But this meeting brought us into what could have been the 50s, 60s, 70s, 80s, and now it could be the same next decade. The science, the general trajectory, where it is, obviously faces this huge problem, smacking us in the face as we speak. And it feels that we should do something about it; we saw it in your wonderful talk.

Back in the what, the 70s, there was a spirit of adventurism and people going out and actually just taking science, and not worrying about publications, but just getting stuff done and finding things out, dragging icebergs to places, which now would be laughed out of this room, suggesting you just tow them around, but we're getting to that point where maybe we do need to do things like that; do we need another thousand more field campaigns to tell us what we already know?

And do we actually need to float around our body of knowledge, which is colossal here, to something proactive? I'm not saying we go and re-seed the atmosphere necessarily, but it's, you know, do we need to start getting on the front foot?

It feels like we're being a bit, I've used the phrase a while, we're being a bit like coroners, you know what I mean? We've heard scientists witnessing the death of all these things and it feels like we need to start getting on the front foot quite rapidly. But maybe that's, I mean that's for other people to do. But there's such a body of knowledge here. This could be used.

[*Pause.*]

**58.00** Minchew: Yeah, I just want to address directly what Geoff said. So there certainly are efforts and people are talking about approaches that we would broadly categorise as geoengineering, and that sort of thing.

And I think that we as a community ought to, at the very least, be aware of what it is that people are talking about and have some part in that discussion: geoengineering on glaciers, or more broadly speaking, any kind of geoengineering of climate.

This sort of thing is going to happen. People are going to try out these methods. And so if we take a kind of back seat to this, and we don't become a part of this conversation, then we're just going to be absent from it and people are just going to go on and do things whether we like it or not. And so, regardless of people's feelings about the extent to which we should actively intervene in changes that are happening within the broader climate system, we need to at least maintain an awareness of the conversation that's happening, particularly in the private sphere. Billions of dollars are going to be poured into this and people are definitely going to try things.

So we should, I think, make a little bit more of an effort to at least actively engage the folks that are pushing forward on that front. So at the very least, maybe we can help to stave off the most irresponsible approaches.

---

[12] Geoff is referring to climate change.

**59.36** Evatt: Be careful. This is something where the IGS could take something and just have a conference or a pointed proceedings, just a workshop, somewhere where we can all just talk incorrectly for a session, get some other people in to speak and just, you know, have a brainstorming, just ask those awkward questions: *can* we do it? The answer might be no, but even in just getting somebody who can get bums on seats. I say it could be something for the IGS to do. 'The second international symposium on iceberg trapping …' [*Chortling.*]

Because it's something where the IGS could maintain its relevance. And I'm not trying to say that the IGS just needs to take it on. But it's, you know, you can all come up with some themes if we carry on. Turn it into a little bit more than engineering. But not the whole subject, of course.

**60.33** Hewitt: Maybe just to say that I think being able to address some of these questions is one of the reasons why I think it's really important to keep looking at detailed processes, to understand how these things actually work.

Because I think the tendency, if the focus is on sea level change, which a lot of it seems to be on at the moment, then I think we are going to go down this route of having basically machine learning models. And I think that it's important to know what physics we should put into those which is important. But I think it's also important to actually understand how, for example, how surface melting works, and how ice lenses form, for instance, so that we're aware of what might change, if we do start disturbing things. I think anything that speaks to understanding the process, the detailed processes, is important.

**61.27** Evatt: What you're saying is it's a portfolio of research we need to do as a community, you're completely correct. But at the moment, really, from an engineering, or from a glaciological point of view, there are not many people doing it, and how many talks on it did we have this week?

So it would be nice to increase that and make it more acceptable within the community to be talking to geoengineers and so on.

**61.51** Hewitt: I guess I didn't really want to get into geoengineering. I just think that the need to be able to respond to suggestions that IGS might work is something that speaks to understanding the process.

**62.04** Guy Kember, Dalhousie University: The Chinese have already started to operate at this point. That's known information. They're already practising and building things.

**62.18** Benn: Okay, so we're now half past the hour, I think that it's probably a timely moment to draw the proceedings to an end, so thank you very much for your contributions to the discussions, and of course, the talks. We have found this enormously stimulating and have been taken to the edge on a number of talks, the edge of reason in some of them, [*laughter*] and lots and lots of food for thought. So thank you, everyone. [*Applause.*]

**Supplementary material.** The supplementary material for this article can be found at https://doi.org/10.1017/aog.2024.42.