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Q1 The European Association for the Study of Diabetes (EASD)'s European study groups cover the full spectrum of diabetes research. How do their meetings complement the main congress, and which scientific developments at EASD 2026, your first congress as Vice President, left you most excited about the future of diabetes research?

More than 20 European study groups, set up by EASD members, cover a broad range of topics related to diabetes, from genetics and islet biology to primary care, eye complications, psychosocial aspects, and health economics. They run independently of EASD and hold small, focused meetings, often of around 100–200 people, where you meet the experts in your field in a very interactive setting. The EASD Annual Meeting, which attracted more than 16,000 participants this year, is the opposite: an opportunity to discover things you wouldn't usually look at. Both are really exciting.

As for the highlights of 2026, the prize lectures were amazing. Klaus Kaestner, University of Pennsylvania, Philadelphia, USA, received the Albert Renold Award and gave a superb lecture on forkhead box A (FOXA) transcription factors in pancreas development. In the second part of his talk, he presented data from the Human Pancreas Analysis Program (HPAP), a National Institutes of Health (NIH)-funded initiative he co-founded. The programme processes donor pancreases from people with and without Type 1 diabetes (T1D) or Type 2 diabetes (T2D) for gene expression, imaging, and chromatin studies. What is amazing is that all of these data are made open access to the diabetes research community. This has really changed the way we do research, because we can now look at the tissue of the person who has the disease.

I also enjoyed the Minkowski Prize Lecture by Zachary Gerhart-Hines, University of Copenhagen, Denmark, on the neurokinin 2 receptor (NK2R). He found that agonists of this receptor increase energy expenditure and decrease food intake: a double hit for body weight regulation.

I was also very excited by the presentations on stem cell-derived β cell replacement. A research team from Peking University, Beijing, China, presented their work on chemically reprogramming somatic cells into induced pluripotent stem cells, rather than using the Yamanaka factors that most labs use. This is close to my own research interests: our laboratory reprogrammes patient cells into induced pluripotent stem cells and differentiates them into pancreatic β cells to study disease mechanisms and test new treatments. The other obvious application is cell replacement. The team from Peking University also transplanted stem cell-derived β cells under the anterior rectus sheath, rather than into the portal vein as is traditionally done for islet transplantation. This matters because there is still a worry that undifferentiated cells could lead to tumours, or teratomas. The anterior rectus sheath is easier to reach and to monitor with ultrasound or MRI.

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Per-Ola Carlsson, Uppsala University, Sweden, also presented work on genetically modified human islets, with human leukocyte antigens (HLA) Class I and II knocked out and *CD47* overexpressed. So far, only one person has been transplanted, in the forearm, which is not an ideal site, but as a proof of concept the results are impressive. I think we may end up combining these two approaches, generating autologous stem cell-derived β cells that are edited to be less visible to the immune system, which would allow us to reduce immunosuppression. These talks beautifully covered basic research to clinical application.

> **These talks covered everything from basic research to clinical application, which was beautiful to see**

Q2 Your research has long focused on pancreatic β cell dysfunction and the mechanisms underlying β cell loss. With β cell preservation and restoration attracting increasing scientific interest, which recent advances do you consider most promising, and how should we go about translating these findings into new therapeutic approaches for patients? How are they influencing your team's future priorities?

When we started differentiating stem cells into β cells, we did it because we thought it would be an extraordinary model for studying disease mechanisms in patient cells. You obtain blood cells from a patient with diabetes and produce that person's pancreatic β cells in a dish. Even after 10 years, that still gives me goosebumps. But along the way, we have made a number of discoveries that could potentially be translated to the clinic, one of which I presented at the Islet Study Group satellite symposium in Milan, Italy.

We started with monogenic forms of diabetes: rare diseases in which a single genetic variant causes a severe, often very early-onset phenotype, because we expected these mutations to have large effects on β cell behaviour in a dish. As our confidence grew, we moved on to variants associated with T2D risk. The first was an intronic variant in *CCND2*, which encodes the cell cycle protein cyclin D2. It is present in 2% of the population, and carriers have a 50% lower risk of T2D, which is a large effect size.

Our hypothesis turned out to be correct: β cells that we genetically modified to carry this variant hypersecrete insulin. This fits with genetic studies showing that carriers have better insulin secretory responses during an oral glucose tolerance test.

This could be quite interesting for β cell replacement in T1D. If a β cell secretes much more insulin, we would need to transplant fewer cells to cure a person, and scalability is a real issue in stem cell-β cell replacement. We have not worked on immune shielding ourselves, but technically these modifications can be combined in a single cell. You could generate what you might call 'super β cells', with advantages coming from different mechanisms.

At the same symposium, I also very much enjoyed the talk by Diego Balboa, University of Helsinki, Finland. We typically genetically modify stem cells and then select the clones of interest, but he presented a novel approach to modify the differentiated end-product with high efficiency.

Q3 Your work on monogenic diabetes and β cell dysfunction has explored mechanisms including endoplasmic reticulum (ER) stress and β cell apoptosis. What can studying these rarer, genetically defined forms of diabetes teach us about more common forms of the disease, and are there lessons that could already inform clinical practice?

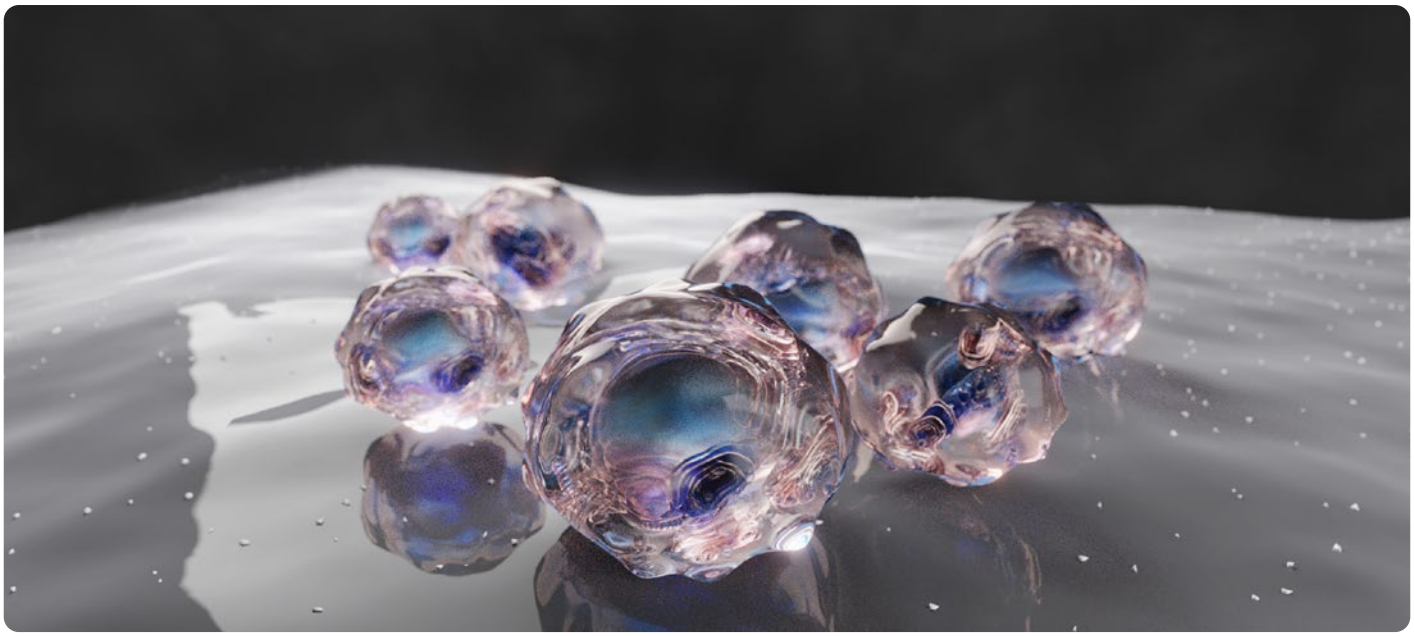
Let me start with a comment about T2D. It is a diagnosis of exclusion: there is not a single diagnostic criterion for it. Everybody pictures diabetes that develops at an older age in people with excess body weight, but it doesn't have to be like that. In some countries, adolescents are now developing T2D, and in my clinic, I see many patients with T2D who are not obese and have markedly reduced β cell function.

What I find frustrating is that, even in 2026, for 85–90% of people with diabetes, we have no way to diagnose T2D in the clinic. Different degrees of β cell failure and insulin resistance all end in hyperglycaemia, and there are probably many ways to develop T2D. If you study islets from people with T2D, as I do, you are unlikely to find a single gene expression signature for the disease.

That said, more and more studies show that ER stress is present in β cells in both T1D and T2D. This may be due to genetic variation, but also to environmental exposures. Our laboratory has shown that pro-inflammatory cytokines, as seen in T1D, induce ER stress and contribute to β cell dysfunction and death. So do free fatty acids, alone or with high glucose, in T2D. These discoveries sparked my interest in monogenic diabetes, because, around the same time, monogenic forms of diabetes related to ER stress were starting to be discovered.

In these children or babies, a single variant that impairs one protein is enough for the β cells to fail very early on in life. ER stress occurs naturally when β cells produce a lot of insulin, and normally the cell copes. What these patients tell us is that these genes, proteins, and pathways are fundamental to β cell development, function, and survival. These conditions are, if you like, caricatures of what may happen in T1D and T2D through a combination of genetic variants and environmental insults, such as dietary factors or viral infections. Even if these monogenic forms of diabetes are very rare, we can learn a lot from them.

Using stem cell-derived β cells from these patients, we study disease mechanisms, then use the gene expression signatures to predict, *in silico*, drugs that could restore normal gene expression. We then test whether those drugs improve β cell function or survival. It is quite possible that some of them will ultimately be useful in T1D or T2D. This is the long and winding road we take, with the ultimate goal of improving treatment for many people with diabetes.



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Q4 Building on these insights into β cell biology and the genetics of diabetes, what are the main barriers to making precision medicine a reality in routine diabetes care? What advances in genetic testing, disease classification, or treatment selection would make the greatest difference for patients?

This is something I try to pass on to early career clinicians. Diagnosing diabetes is easy; measuring blood glucose is not complicated. Differential diagnosis, however, is not practiced enough. Many clinicians will not think further than: "This person developed diabetes at 3 years of age, so it must be T1D; this person developed it at 50, so it must be T2D." For me, the first step to be taken is that differential diagnosis becomes routine practice.

T2D is a diagnosis of exclusion that we currently don't call by any other name; polygenic risk scores don't perform well. For T1D, however, we have β cell autoantibody testing, C-peptide to assess residual insulin secretion, HLA risk assessment, and polygenic risk scores that work very well. A misconception is that T1D is a disease of children; in fact, more people develop it in adulthood than in childhood.

To help you identify monogenic diabetes, free tools are available. Beverley Shields and Andrew Hattersley, University of Exeter, UK, developed the Maturity-Onset Diabetes of the Young probability calculator. This online app uses basic clinical information to estimate the probability that a person has one of the more common forms of monogenic diabetes. Taking a patient's family history is also free of cost. Sometimes you see people with multiple generations of diabetes in their family, and nobody has ever considered whether this might not be T1D or T2D.

Genetic testing has become much more accessible. Where we used to request *HNF1A* testing, we now do whole exome or even whole genome sequencing. A genetic diagnosis can have a major impact on management: for some monogenic forms, people can switch from insulin to sulfonylureas and achieve much better glucose control. It also allows genetic counselling and testing of family members, who may have exactly the same disease without it being recognised.

So, one of the main barriers to precision medicine is clinical implementation. Beyond that, for the 85–90% of people with diabetes who have T2D, we lack diagnostic criteria and the evidence that T2D subtypes should be treated differently remains limited. What remains unsolved is why a particular person develops T2D, and whether we can offer them something personalised for their pathway to the disease.

Q5 As Vice President of EASD, what would you most like HCPs working in diabetes to take away from this year's Congress? If they could make one change to their clinical practice following EASD 2026 to improve the care they provide, what would you encourage them to think about?

I would encourage people to think about precision medicine and what it will take to get there, rather than taking a one-size-fits-all approach. There is a lot of excitement about glucagon-like peptide-1 receptor agonists as well as dual, triple, and quadruple agonists, but to what extent these drugs are truly disease-modifying remains unaddressed. I don't want to end on a negative note, but there is still a lot to do. I would encourage every clinician to ask why the person in front of them developed diabetes, and whether there is anything to suggest that it is not T2D. If we identify an alternative aetiology, we may be able to offer better treatment. The other thing exciting change is the early diagnosis of T1D, which Anette-Gabriele Ziegler, Helmholz Munich, Oberschleißheim, Germany, beautifully described in her amazing Claude Bernard Award Lecture. Her work shows that screening works: we can now diagnose T1D in children and adults before hyperglycaemia develops. That opens a window of many years in which we can intervene to prevent, or at least delay, the disease with the therapies now being tested and brought to market. Some countries are moving beyond screening first-degree relatives to population-wide screening. If we get to a stage where we can prevent T1D, that is going to be amazing.

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