DISCUSSIONS

Phase behaviour and thermodynamics: general discussion

Andrew Abbott, Hiroshi Abe, Leigh Aldous, Rob Atkin,
Magdalena Bendová, Matteo Busato, José Nuno Canongia Lopes,
Margarida Costa Gomes, Benjamin Cross, Carin Dietz, Jeffrey Everts,
Millicent Firestone, Ramesh Gardas, Matthieu Gras,
Tamar Greaves, Simon Halstead, Christopher Hardacre,
Jason Harper, John Holbrey, Johan Jacquemin,
Philip Jessop, Doug MacFarlane, Florian Maier, Himani Medhi,
Markus Mezger, Agílio Pádua, Susan Perkin, Joshua E. S. J. Reid,
Satyen Saha, John M. Slattery, Morgan L. Thomas,
Shraeddha Tiwari, Seiji Tsuzuki, Betul Uralcan, Masayoshi Watanabe,
James Wishart and Tristan Youngs

Agílio Pádua opened the discussion of the introductory lecture by Doug Mac-Farlane: Why are phosphonium/phosphate ionic liquids such good lubricants? I understand they tend to form ion pairs in the bulk liquids because both cation and anion contain a charged center surrounded by long alkyl chains, resulting in an "oily" character. Is it known if these aggregates remain in the interfacial layers with the metal, or if the ions pairs dissociate and it is single ions that attach to the surface?

Doug MacFarlane replied: In low dielectric constant media such as base lubricant oils, the degree of dissociation is unlikely to be very high. At the surface there may be specific adsorption of one of the ions in particular, depending on the charge or functionality of the surface, however the counterion is likely to remain in close association in the secondary layer.

Andrew Abbott asked: Concerning the diagram correlating conductivity *vs.* composition we have previously suggested that the difference between an ionic liquid and an ionic solution is that the charge changes from being dominated by hole mobility to ion mobility.[1] Does your description support this idea or move it to a point where ions cluster. On another point: Your assumption ignores the issues of heterogeneity. Work in the literature suggests pools of water in ILs and *vice versa*, how does this fit into the model?

1 A. P. Abbott, K. S. Ryder, P. Licence and A. W. Taylor, *What is an Ionic Liquid? In Ionic Liquids Completely UnCOILed*, ed. Plechkova and Seddon, John Wiley and Sons, Hoboken, NJ, USA, 2015.

Doug MacFarlane answered: There is nothing inconsistent about the various models of conduction in ILs, and those for dilute solutions, and the overall trends discussed here. The interesting challenge is to adapt these models to address the changes that take place as we add diluents to the IL and in particular the variety of structural heterogeneities that certainly exist in the mixture domain. The latter of course depends critically on the structure of the ionic liquid ions, as well as the molecular liquid involved, and a generic model (therefore probably quite qualitative at the outset) needs to be able to accommodate this diversity.

José Nuno Canongia Lopes asked: I would like to comment on the studies just presented on the synergistic combination of two ions to enhance a given property. I have been working on the explanation of hydrotrope effects using MD simulations.[1] In those cases the use of ionic liquids can increase ten-fold or more the solubility in water of an otherwise almost insoluble organic solute. The ionic liquid ions work synergistically: the short alkyl chains present in the cation interact with the organic solute, the anion is chosen to be hydrophilic and interacts with the aqueous phase. Since the ionic liquid ions will be interacting with each other (mainly *via* electrostatics), they will establish a link between the organic solute and the aqueous media. The ionic liquids used generally have alkyl side chains shorter than six carbon atoms which means that no micelle formation is involved in the process.

1 A. F. M. Cláudio, M. C. Neves, K. Shimizu, J. N. Canongia Lopes, M. G. Freire and J. A. P. Coutinho, *Green Chem.*, 2015, **17**, 3948–3963.

Doug MacFarlane responded: I think that is a very nice example of tuning the two ions towards a certain set of properties with regard to the solubility of third components in molecular solvents.

Markus Mezger commented: Fig. 1 in your paper (DOI: 10.1039/c7fd00189d) nicely points out the different structural and dynamic regimes found in electrolytes ranging from strongly diluted solutions to pure ionic liquids. It provides a valuable summary that can help in understanding their specific properties. For strongly diluted solutions we have an established theory. Different extensions have been developed to take into account corrections that get relevant at higher ion concentrations. On the other hand, over the last few years there has been a significant increase in the understanding of pure ILs *i.e.* systems that are entirely composed out of ions. Therefore, an interesting question is whether there is a distinct concentration dividing the region where an electrolyte should typically be consider to be primarily composed of ions rather than being a highly concentrated salt solution. One suggestion might be to consider the Kirkwood transition at which, in the limit of large distances, the charge correlation function changes from monotonic to oscillatory. Where would this transition be located in your graph and for which properties could it serve as a guide to estimate boundaries between different regimes? For example, I could imagine that in particular for processes near interfaces such as in electrochemistry or the formation and stabilization of nanomaterials changes might be observed around the Kirkwood transition.

Doug MacFarlane answered: I think that the Kirkwood transition indeed represents an important distinction between the two extremes of dilute electrolyte and pure liquid salt. Where there is strong ion association (pairs and larger aggregates) at dilute concentrations, for example in the lubrication example discussed, the charge correlation functions may show signs of both regimes, *i.e.* strongly oscillatory behaviour over the first few ion diameters and then dropping away very rapidly thereafter. I think MD studies of such systems would be very valuable in further exploring this boundary under these conditions. Certainly this type of behaviour will be modified by the presence of a surface, or a charged electrode, and therefore the "boundaries" may then shift.

Joshua E. S. J. Reid questioned: In your paper, you highlighted that the combination of two bioactive components, typically paired with inorganic counterions, had improved transport effectiveness through a lipid bilayer. Does this particular combination of lidocaine and ibuprofen form a protic salt or is it better described as a hydrogen bond adduct, and as such, what are the effects of the extent of proton transfer on this increased bioactivity?

Doug MacFarlane responded: This is a very important issue and one that depends strongly on the nature of the molecular component involved in the mixture (also including the interior of a lipid bi-layer for the moment as a molecular liquid-like domain). Also it depends on the relative acid and base strengths of the two species. Recognising the existence of "ion-pairs" in the solution, the question then amounts to inquiring about the extent of proton transfer from acid to base; with strong transfer we have an ion pair, if it is weak we may effectively have a H-bonded adduct instead. In both cases the molecular solvent properties obviously have a strong influence on whether this complex remains intact, as opposed to dissociating into separate species. The distinction between these two states can be quite minor structurally, though the additional electrostatic interaction in the ion-pair may be important in the overall ion-pair binding energy in that case. To the extent that membrane transport in particular is a key aspect of bio-availability then the formation of these bound species is the key step and whether the proton is transferred may not be critical. Between work from our own laboratories and that from the Rogers group we have shown examples of both kinds of behaviour with useful effect.

James Wishart noted: In the discussion of Fig. 1 in your paper it was remarked that the transitions from dilute "salt-in-solvent" behavior to "solvent-in-salt" behavior as the mole fraction of the ionic liquid increases occur at quite small ionic liquid mole fractions. If the data were viewed in terms of volume fractions the transitions between the two regimes might seem more intuitive, owing to the molar volume differences between the ionic liquids and the solvents.

Doug MacFarlane responded: Indeed, the fact that ionic liquid molar volumes (ion sizes) span such a huge range is a significant feature in thinking about diagrams like Fig. 1. The graphics in Fig. 1 visualise the ions and molecular species as not very different in size. But the ions in cetrimonium nalidixate for example are considerably larger and therefore a given mole fraction of salt corresponds to a much larger volume fraction of salt and the transition may appear more in the middle of the plot that uses a vol% scale.

Susan Perkin said: I appreciate very much your encouragement that we turn our attention to the properties of ionic liquids mixed with solvent, and the spectrum of electrolyte properties from the dilute limit to the pure ionic liquid limit. A lot will be learned from this way of thinking. In the same way that our ionic liquid community has learned from much earlier work on molten salts, we must now inspect and learn from the prior work on highly concentrated electrolytes. One relevant concept arising in the past, relevant to your discussion of ion association and ion pairs, is the idea of redissociation. Ion pairs present in low dielectric solvents at moderate concentration may redissociate at higher concentration. Do you think this phenomenon is apparent in any of the physical measurements you have presented today?

Doug MacFarlane answered: Thank you for emphasizing the point that there is much to be learned from the well developed understanding of electrolyte solutions (in water and other solvents). As you say, as these models are extended into concentrated solutions, they first recognise the important role of ion pairing (more significant in low dielectric solvents, but nonetheless important in aqueous) and at still higher concentrations the role of re-dissociation, *i.e.* the growing significance of the several possible equilibria of the type:

$$2(\text{CatAnion}) = [\text{Cat}_2\text{Anion}]^+ + \text{Anion}^-$$
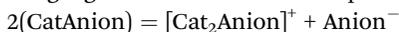
As written this shows the formation of ionic species from neutral ion pairs and the molar conductivity passes through a minimum in concentration and begins to rise again when this happens. This is more commonly observable in low dielectric solvents as shown in our paper in the data set based on dichloromethane. Typically at still higher concentrations the molar conductivity then passes through a maximum as the overall species mobility begins to fall towards that of the pure IL and T_g begins to rise.

So, returning to your original point, it is indeed important for us to recognise and absorb this relatively well developed understanding of concentrated salt solutions. What the ionic liquids community brings to this interesting region of two component mixtures is the ability to manipulate the functionality of the ions and an understanding of how this impacts on the ion pair binding energy which ultimately controls the concentration dependence of the equilibrium above. The cetrimonium nalidixate salt discussed, in the paper, is an example where strong ion-ion interactions are clearly present even in water pushing the molar conductivity to very low values, but also revealing useful properties as an ion pair (or ion-aggregates) that the inorganic salts of the ions do not show.

Millicent Firestone opened discussion of the paper by Calum J. Drummond†: In your work you examine the SAXS patterns determined for C-6 through C-16 and observe solely lamellar structures. Can you comment on why the structure appears to be invariant as a function of chain length? It is interesting that you do not observe higher order structuring with increasing chain alkyl chain length?

Tamar Greaves responded: These alkylammonium nitrate or formate salts only formed lamellar liquid crystal structures. They can be described as having

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† Calum J. Drummond's paper was presented by Tamar Greaves, RMIT University, Melbourne, Australia.

a cylindrical shape due to the relative proportions of the ammonium ionic group and the unbranched saturated alkyl chain. We envisage that a significantly bulkier head group could potentially induce higher order normal phases, while branching or saturation of the alkyl chain could potentially lead to higher order inverse phases. The critical packing parameter (CPP) $= V/al$, where V is the effective volume of the hydrocarbon chain, a is the effective head group area and l is the effective length of the hydrocarbon chain. For these ionic liquids CPP is approximately equal to 1, since they are all lamellar. The ionic headgroups are not changing for either the formate or nitrate series and hence a is invariant with chain length. Consequently for CPP to stay relatively constant it means that V and l are scaling similarly with n.

Millicent Firestone added: As a follow up to my prior question, have you tried to calculate the head to tail ratio for the C-6 to C16 ILs to estimate a critical packing parameter?

Tamar Greaves replied: We have not estimated a critical packing parameter for these ionic liquids. They all formed lamellar phases which indicates they have a critical packing parameter close to 1. In addition, due to being a thermotropic liquid crystal phase, and not a dilute amphiphile in solvent, the global constraints imposed by the surrounding species need to be taken into consideration, rather than simply the geometry of the amphiphiles.

José Nuno Canongia Lopes commented: This is a follow-up remark to the previous question. I am not sure that the structures presented are interdigitated. The spacings are indeed smaller than two fully stretched alkyl side chains, but this could just mean that the chains are tilted relative to the ionic planes or present some level of *gauche* conformer. In order to check the different hypotheses one should consider for instance the areas/volumes occupied by the alkyl chains relative to the space occupied by the ionic surfaces or conduct MD simulations to get a better notion of the structure of this highly anisotropic but still liquid phase.

Tamar Greaves responded: The spacings for the alkylammonium nitrates and formates are consistent with the spacings being smaller than two fully stretched chains. This could be due to interdigitation of the chains, chain tilt, *gauche* conformers or a mixture of these. We agree that additional information from MD simulations would be necessary to fully characterise the structures present.

Markus Mezger queried: Your work includes scattering data and thermal measurements for a wide range of different ILs. Together with other measurements, it therefore might be extremely valuable to identify universal links in the structure–property relationship of these ILs. The trends observed for the existence of liquid crystalline (LC) mesophases, as well as the cryst-LC and LC-iso phase transition temperatures are primarily discussed in terms of specific intermolecular interactions. This includes the nitrates and formates measured in this work as well as the results from other anions such as Cl⁻ referenced in the discussion and conclusion section. Recently, the authors in ref. 1 below showed that the thermophysical behaviour of a set of Br, ClO_4, $N(CN)_2$, CF_3COO, NO_3, BF_4, PF_6,

CF_3SO_3, CF_3SO_2NCN and $N(CF_3SO_2)_2$ based ionic liquid crystals with different cation alkyl chain lengths can be described surprisingly well using a volume-based approach.[1] To what extent are your results consistent with this approach and where are the differences observed?

1 Y. V. Nelyubina, A. S. Shaplov, E. I. Lozinskaya, M. I. Buzin and Y. S. Vygodskii, *J. Am. Chem. Soc.*, 2016, **138**, 10076–10079.

Tamar Greaves answered: In the paper by Nelyubina *et al.* 1-alkyl-3-imidazolium ionic liquids (ILs) with different anions are looked at which form liquid crystal phases. The relationship between the volumes of the cations and anion with the minimum alkyl chain length on the cation required for a liquid crystal phase to be present was investigated, along with the temperatures for melting points, clearing points and mesophase ranges. If we classify the anions purely based on their size with nitrate > formate > chloride, then we do not see any strong correlations such as those reported by Nelyubina. It is probably not valid to assume that the findings for ILs with the 1-alkyl-3-imidazolium cation will be valid for ILs containing alkylammonium cations, or more generally between aprotic and protic ILs. The interactions between the alkylammonium cation and different anions are very different compared to those for the imidazolium containing ILs. To a large extent this is due to the hydrogen bonding present for protic ILs, and the arrangement of ions, such as has been discussed by Hayes *et al.*[1] Consequently, it is difficult to consider how the protic ILs can be purely described by volume.

1 R. Hayes, S. Imberti, G. G. Warr and R. Atkin, *Angew. Chem., Int. Ed.*, 2013, **52**, 4623–4627.

Shraeddha Tiwari asked: According to your interpretation, the SAXS data of the ionic liquids indicate the presence of interdigitated chains in the ionic liquids. The phase transition from solid phase to liquid crystalline phase, which primarily involves the perturbation of the alkyl side chains of the ionic liquids, should depend on the nature and strength of the interaction between the alkyl side chains. What inference (if any) can be drawn from the solid to liquid crystal transition temperatures about the nature, extent and strength of such interdigitation interactions?

Tamar Greaves responded: The spacings for the alkylammonium nitrates and formates are consistent with spacings smaller than two fully stretched chains. As indicated in response to an earlier question, this could be due to interdigitation of the chains, chain tilt, *gauche* conformers or a mixture of these. We agree that the phase transition between solid to liquid crystal is predominantly due to the melting of the alkyl chains of the ionic liquids. We believe that additional information, such as MD simulations, would be required to rigorously characterise whether there is interdigitation, chain tilt or *gauche* conformers present.

Ramesh Gardas remarked: What was the extent of proton transfer for the synthesized series of protic ionic liquids? Is it >95% for all PILs, how did you measure it especially when acid to base stoichiometry is not 1 : 1?

Tamar Greaves answered: The cation : anion ratio was measured using ^{1}H-NMR for the alkylammonium formate ionic liquids, and is provided in Table 2

in our paper (DOI: 10.1039/c7fd00148g). The ratios vary between 0.81 and 1.22 for the formate series. The exception was pentadecylammonium formate which had a ratio of 0.41, and this salt was identified as only providing data "indicative at best". The cation : anion ratios for the nitrates were not measured, though they were assumed to have a higher degree of proton transfer than the formates due to the nitric acid precursor being a significantly stronger acid than formic acid.

Jason Harper questioned: This is related to the last question and focuses on the determination of ratios of species using NMR spectroscopy. Did you look at the relaxation times of each of the species? The recycle time listed is very short (1 s) (*cf.* standard of 5 s, which is not sufficient even for organic systems). Typical recycle times used are in the order of $5 \times T_1$.

Tamar Greaves replied: ^{1}H-NMR spectra were acquired for hexadecylammonium formate using relaxation times of 1, 2, 5, 10 and 25 s with 32 scans per sample. The ratio obtained using these different times had no consistent variation with time, and had a variation of 10%. We agree that longer relaxation times would probably lead to more accurate ratios, though we do not think they would have a significant impact on the ratios obtained for these samples.

Johan Jacquemin asked: The claimed water level in some "pure" ILs is sometime up to 0.50 in water mole fraction units, could you comment on the effect of water level on the uncertainty in the reported phase transition temperatures? Did you dry all ILs prior to use? By looking at Fig. 2a in your paper, it may be interesting to investigate the thermal properties of the C5N, because without such data you cannot claim that the liquid crystal phase is occurring when $n \geq 6$ in the case of the *n*-alkylammonium nitrates.

Tamar Greaves answered: We have included the water contents for the alkylammonium formates and nitrates and the cation : anion ratios for the alkyl ammonium formates to be clear about the composition of the ionic liquids being investigated. We recognise that the water contents vary between the ionic liquids used, and was particularly high for decylammonium nitrate and tridecylammonium formate. It can be expected that the water content will cause some variation in the thermal transition temperatures. As described in the paper, the ionic liquids were all dried on a rotary evaporator, followed by drying on a freeze dryer. Due to variations in hydrophilicity, sample quantities and drying times, the exact drying conditions do vary between samples. In addition, we did not want to conduct rigorous drying due to the risk of increasing non-stoichiometry of the samples, which would occur for any with weaker proton transfer, or cause amide formation, where the rate of amide formation is increased with increasing temperature.

Johan Jacquemin said: Data reported in the literature (Fig. 2b of your paper) look very strange particularly when $n < 4$ in the case of *n*-alkylammonium formates. Could you comment on the accuracy of these data?

Tamar Greaves responded: In Fig. 2b in the paper the melting point of the alkylammonium formates for n less than 4 has a large variation. In part this is because we included the literature data for propylammonium formate which has been given as both 50 °C and −55 °C. There is also possibly an odd-even alkyl chain length effect leading to variation of the melting point in this region.

Philip Jessop opened a general discussion of the paper by Agílio A. H. Pádua: Thank you for the presentation, which I found particularly interesting. For the exfoliation process, you mentioned the importance of an aromatic group in the cation. Is a similar trend found in molecular solvents (*i.e.* do aromatic molecular solvents do a better job at exfoliation than non-aromatic solvents)? Do you anticipate that an aromatic group in an ionic liquid's anion would also be beneficial? What other factors might help, like mixing a molecular solvent with an ionic liquid, to increase the rate of diffusion inside the liquid phase? Or maybe increasing the speed of mixing to enhance mass transfer rates?

Agílio Pádua replied: Thank you. In molecular solvents the aromatic character does not work by itself. For example, toluene is not a particularly good solvent for exfoliating graphene. A higher degree of polarity seems to be necessary (*e.g.* benzonitrile). Pyrrolidones are among the best solvents and are not aromatic. Having the aromatic substituent on the anion would be slightly different because cations have a favourable interaction with the pi-system of graphene whereas anions prefer the edges. However, in extended graphene/graphite surfaces both cations and anions are present. So I would expect that aromatic groups on the anion would improve exfoliation.

And yes, although I did not perform simulations of graphene exfoliation in IL-molecular solvent mixtures, we did that for the dissolution of cellulose and lowering viscosity and improving mass transfer improves the dissolution process.

Susan Perkin asked: My question concerns the way in which you set up the simulation for graphene exfoliation. In the simulation, you pull on one end of the top sheet, which bends upwards, and then peels off the stack. An alternative way to separate the layers in a simulation might be to separate the sheets in a plane-parallel way. It seems that using your peeling method gives information on the interfacial energy, since (once bending has occurred) the potential of mean force is simply telling about the energy to create new solid–liquid interface. Plane-parallel separation would give information about the intercalation of the liquid into a narrow slit, and it's variation with slit width. Would you expect the energy you measure to be related to this, and how do each of these mechanisms relate to the expected mechanism by which graphene is exfoliated in liquids?

Agílio Pádua responded: We have done calculations of peeling one layer of the 2D nanomaterials in molecular solvents using the plane-parallel arrangement, see the references below.[1,2] In this arrangement the potential of mean force (PMF) initially has a very steep rise because we are pushing against the cohesive energy of the solvent, whose molecules haven't yet intercalated into the layers. When they do, they form a highly ordered confined monolayer. Therefore the PMF profile has large signals due to these strong forces and is not so sensitive to the subtle balance of van der Waals forces between sheets and with the solvent. We thought

that the peeling procedure we implemented is directly sensitive to these delicate interactions. Besides, a steady state is quickly attained where the separation/intercalation front advances. This route is actually more sophisticated since we have to take into account the natural flexibility of the material.

1 V. Sresht, A. A. H. Pádua and D. Blankschtein, *ACS Nano*, 2015, **9**, 8255–8268.
2 V. Sresht, A. G. Rajan, E. Bordes, M. S. Strano, A. A. H. Pádua, and D. Blankschtein, *J. Phys. Chem. C*, 2017, **121**, 9022–9031.

Markus Mezger commented: I would expect that the exfoliation efficiency depends on two factors. First, solvent molecules have to get inside the slit pores. Second, solvent molecules adsorbing onto the freshly cleaved surfaces lower their interfacial energies. The first process is a dynamic one. Here energy barriers, related to intermediate structures such as separated dissociated ion pairs, can play an important role. In the second step, specific interactions between the 2D nanomaterials and molecular moieties of the solvent such as aromatic groups strongly contribute to the final static energy balance between the starting material and the fully separated sheets. To what extent can your simulations help to separate the contributions from these static and dynamic effects for the overall exfoliation efficiency of a solvent?

Agílio Pádua answered: The calculation of the potential of mean force (PMF) is made at a series of fixed separations at which the system is first equilibrated and only then is the force acting on the sheets measured for a duration sufficient for adequate statistics. So the PMF calculation is static. We check that going in the opposite order. I apologise if showing a movie of the peeling process obtained from a molecular dynamics trajectory has given the impression that the PMF is performed dynamically. We only distinguish between the energetics of separating the surfaces of the material in the absence of solvent, due to interlayer and flexibility contributions (by performing a run in vacuum) from the contributions of the liquid medium. Unfortunately in this setup we could not resolve the two dynamic mechanisms that you describe, although it would be interesting to investigate them.

Seiji Tsuzuki asked: My question concerns the interaction between the ionic species and graphene. If the ionic species come close to graphene opposing charges (mirror charge) are gathered near the ionic species. The coulomb interaction between the ionic species and mirror charge is substantial. This interaction will affect the distributions of ionic species near the graphene. What consideration was given to the polarisation of graphene due to the vicinity of the charged ions?

Agílio Pádua responded: Yes, ions and polar molecules induce electronic polarisation on the graphene. In smaller-scale simulations we took account of polarisation explicitly,[1,2] although not by using the mirror-charge method. In fact, the mirror-charge applies to perfect, flat surfaces and to other simple geometries but is not transferable to surfaces with atomic detail or roughness. Also, it is not simple to define where exactly to put the "mirroring" surface. Therefore, we used a method from the literature[3] in which a specially parameterised dipole is added

to each atom so that the interaction energy of a probe charge with a conducting surface is reproduced. This model is atomistic and therefore can be used for rough surfaces, nanoparticles, *etc.*

The present simulations are large-scale and explicit treatment of polarisation would be computationally too costly, therefore polarisation is included in an implicit way through our parameterisation of interactions between ions and graphene based on *ab initio* calculations.[4]

1 A. S. Pensado and A. A. H. Pádua, *Angew. Chem., Int. Ed.*, 2011, **50**, 8683–8687.
2 A. C. F. Mendonça, P. Malfreyt, and A. A. H. Pádua, *J. Chem. Theory Comput.*, 2012, **8**, 3348–3355.
3 F. Iori and S. Corni, *J. Comput. Chem.*, 2008, **29**, 1656–1666.
4 J. Szala-Bilnik, M. F. Costa Gomes and A. A. H. Pádua, *J. Phys. Chem. C*, 2016, **120**, 19396–19408.

Seiji Tsuzuki asked: Is it possible to include mirror charge interactions in the MD simulations? Did you have any thoughts to include the effects of the mirror charge interactions to the simulations sufficiently accurately?

Agílio Pádua replied: Please refer to my answer to your previous question.

José Nuno Canongia Lopes opened a general discussion of the paper by Hiroshi Abe: You have shown a large difference between the mono-iodide ionic liquids and their trioxide counterparts in terms of their tendency to form amorphous or crystalline solids. Is there any reason for this behaviour? Is there an interpretation from a molecular point of view based on theoretical models or simulations? Can you give us your opinion or some kind of explanation?

Hiroshi Abe answered: The liquid structure of I^- systems has been investigated experimentally[1] and theoretically.[2,3] I^- is surrounding the $C_n$mim$^-$ cation anisotropically. At low temperature, I^- randomly freezes at the energetically equivalent sites.

In contrast, I^{3-} is large enough. Then, stabilization site could be restricted. Near the C2 position of the imidazolium ring, I^{3-} could be stabilized even in the liquid state. Then, $[C_3$mim$][I_3]$ is easily crystallized without the positional ambiguity of I^{3-}.

1 C. Hardacre, J. D. Holbrey, M. Nieuwenhuyzen and T. Youngs, *Acc. Chem. Res.*, 2007, **40**, 1146–1155.
2 S. Tsuzuki, R. Katoh and M. Mikami, *Mol. Phys.*, 2008, **106**, 1621–1629.
3 M. H. Ghatee, A. R. Zolghadr, F. Moosavi and Y. Ansari, *J. Chem. Phys.*, 2012, **136**, 124706.

Betul Uralcan continued the discussion of the paper by Agílio A. H. Pádua: This is very nice work that could provide a guide to the rational design of solvent systems for efficient exfoliation of layered systems. I was wondering if you have considered studying or are currently performing simulations with reduced graphene oxide?

Agílio Pádua replied: For the moment we have worked with different 2D materials in their pristine form: phosphorene, MoS_2, graphene, fluorographene. The difficulty with reduced graphene oxide or other forms is to obtain a precise

description of the condition of the material: how many functional groups, of what kind and with what distribution? What about defects? What functional groups or dangling bonds are found on the edges? Without these detailed informations it is difficult to say if the simulation results are meaningful. The same is true, for example, concerning the chemical nature of a metallic surface when studying lubrication using molecular simulations.

Betul Uralcan commented: The reason I asked the previous question was that at one point we were also interested in using reduced graphene oxide (rgo) models in our simulations, but as you said, it was challenging to identify a realistic model that integrates functional groups and lattice defects (vacancies/edges). Specifically, I think I had the rgo structures (it is referred to as functionalized graphene sheets in that study) used in Zhang *et al.*'s AIMD simulations[1] and a possible force field parametrisation from the AIMD data in my mind. And I was just curious about your opinion.

1 C. Zhang, D. Dabbs, L.-M. Liu, I. Aksay. R. Car and A. Selloni, *J. Phys. Chem. C*, 2015, **119**, 18167–18176.

Johan Jacquemin returned to the discussion of the paper by Hiroshi Abe: Very nice work and very interesting results. Have you considered investigating the variation of transport properties of the investigated ILs with pressure? Similarly, Raman spectroscopy under high pressure could also help you to analyze in greater detail the structure changes of your solutions as a function of temperature, pressure and composition. Such an analysis could certainly help you to fine tune the coordination changes of iodine in the IL.

Hiroshi Abe replied: Using a diamond anvil cell (DAC), high-pressure (HP) Raman spectroscopy is generally carried out, combined with ruby tips as a pressure marker. But, in the case of $[C_3mim][I_m]$, the sample became dark brown. Then, we could not monitor the fluorescence of the ruby line as a pressure marker. This is a serious problem of DAC under HP. Thus, in this study, we performed HP X-ray diffraction using a different type of pressure cell. However, we did not give up on measuring HP Raman spectra. As you mentioned, Raman spectroscopy is powerful for detecting the polyiodides. Now, we are considering monitoring the R1 fluorescence by manufacturing a new type of DAC.

Johan Jacquemin said: By looking at the DSC traces reported in Fig. 2 of your paper (DOI: 10.1039/c7fd00172j), are you certain that you have two melting points for the IL with $m = 3.66$? Are you certain that the first melting point is not due to the presence of a eutectic? In this case, this binary system (*i.e.* $C_3mimI + I_2$) could be classified as a peritectic phase diagram containing an amorphous region?

Hiroshi Abe responded: In a previous paper, it was reported that a liquid crystal appeared upon heating.[1] But, in the case of $m = 3.66$, excess iodine might fluctuate a lot. I suppose that compositional fluctuations are not ignored in the typical non-equilibrium system. So, it is possible that eutectic-like decomposition occurs such as iodine-poor and iodine-rich regions. Also, a peritectic phase might be formed by strong fluctuations and nano-heterogeneity. We have already shown

the iodide-induced nano-heterogeneity.[2] We have to consider carefully the behaviors of polyiodides in the "kinetic" phase diagram.

1 V. K. Thorsmølle, J. C. Brauer, S. M. Zakeeruddin, M. Grätzel and J.-E. Moser, *J. Phys. Chem. C*, 2012, **116**, 7989–7992.
2 H. Abe, M. Aono, T. Kiyotani and S. Tsuzuki, *Phys. Chem. Chem. Phys.*, 2016, **18**, 32337–32344.

James Wishart remarked: In your paper you interpret the two closely-spaced endothermic peaks in the DSC trace for non-stoichiometric $[C_3mim][I_{3.66}]$ as melting transitions of two separate phases, one stoichiometric and one with an excess of iodide. Even in pure, stoichiometric ionic liquids, closely-spaced melting transitions are not uncommon and they are often interpreted as originating from metastable crystalline forms created during the thermal history of the DSC scan, during cold crystallization for instance. Cold crystallization is actually observed in this case. Therefore I would like to ask if you have considered that $[C_3mim][I_{3.66}]$ could be behaving as a single phase, and whether you have other forms of evidence that support the multiple-phase interpretation.

Hiroshi Abe responded: That's a good point. Even in pure RTILs, as you pointed out, multiple melting was observed as a non-equilibrium state. The RTILs themselves have an intrinsic instability, derived from fluctuations and the nano-heterogeneity. In case of $m = 3.66$, We have already observed a pre-peak representing the nano-heterogeneity, although the pure one had no pre-peak.[1] So, I speculate that iodine-poor and iodine-rich regions are formed dynamically. To prove the dynamic nano-heterogeneity accompanying compositional fluctuations, we will carry out the quasi-elastic neutron scattering experiments in J-PARC in the near future. Using the obtained $S(q, \omega)$, we can say something.

1 V. K. Thorsmølle, J. C. Brauer, S. M. Zakeeruddin, M. Grätzel and J.-E. Moser, *J. Phys. Chem. C*, 2012, **116**, 7989–7992.

Rob Atkin returned the discussion to the paper by Agílio A. H. Pádua: In our recent work we proposed that step edges were important for intercalation.[1] Your simulation uses a stack but does not include step edges. Is it possible to simulate a step edge? While it is true that you are unlikely to see our "spontaneous intercalation" on your time scale, it may be the energy you calculate is reduced. Based on our results, we would predict lower energy for C_2MI_m TFMS and C_2MI_m acetate.

1 A. Elbourne, B. McLean, K. Voïtchovsky, G. G. Warr and R. Atkin, *J. Phys. Chem. Lett.*, 2016, **7**, 3118–3122.

Agílio Pádua responded: Yes, it is perfectly possible to simulate a step edge and would not be more complicated than what we reported in this paper (DOI: 10.1039/c7fd00169j). This is an excellent suggestion for further work. I expect that the description of the chemical nature of the edge becomes more important because it will affect which ions are present in their vicinity and in what configurations, these details become more relevant if we focus on that specific region. In order for the simulation results to be meaningful we'd need a precise description of what defects and/or chemical functional groups are found on the edges.

Masayoshi Watanabe continued the general discussion of the paper by Hiroshi Abe: You showed that bond lengths of I_5^- are different depending on each I–I bond, based on computer calculation. However, in the liquid state the bond lengths appear to be fluctuating. We have observed that molar conductivities of $[C_3mim][I]/[C_3mim][I_3]$ mixtures with the same viscosity exhibit a maximum (see Fig. 1). Interestingly, the molar conductivity of the mixtures becomes higher than that of the ideal Walden line based on 1 M KCl aqueous solution, although the molar conductivity of $[C_3mim][I]$ is lower than the line. This indicates that the Grotthuss-like hopping (exchange) mechanism works for the transport of these mixtures. Furthermore, the molar conductivity of $[C_3mim][I_3]$ itself is also higher than the ideal line. These experimental results indicate that the structures of I_3^- and possibly I_5^- are not static but fluctuating due to the exchange reactions. The figure also indicates that the exchange frequency also changes depending on the mixing ratios of $[C_3mim][I]$ and $[C_3mim][I_3]$. Structure fluctuation of polyiodide ions may affect the crystallisation dynamics of your systems. How would it affect phase behaviour?

Hiroshi Abe replied: As you mentioned, the bonding distance of the poly-iodides is thermally fluctuating. I really agree with the Grotthuss-exchange mechanism of the polyiodides, which is applied to the $[C_3mim][I_3]$ in the real system. Thus, we have a plan to perform neutron quasielastic scattering experiments in J-PARC in order to clarify the size and life time of the polyiodides. $S(q, \omega)$ of neutron quasielastic scattering can prove directly the dynamic behaviors of the
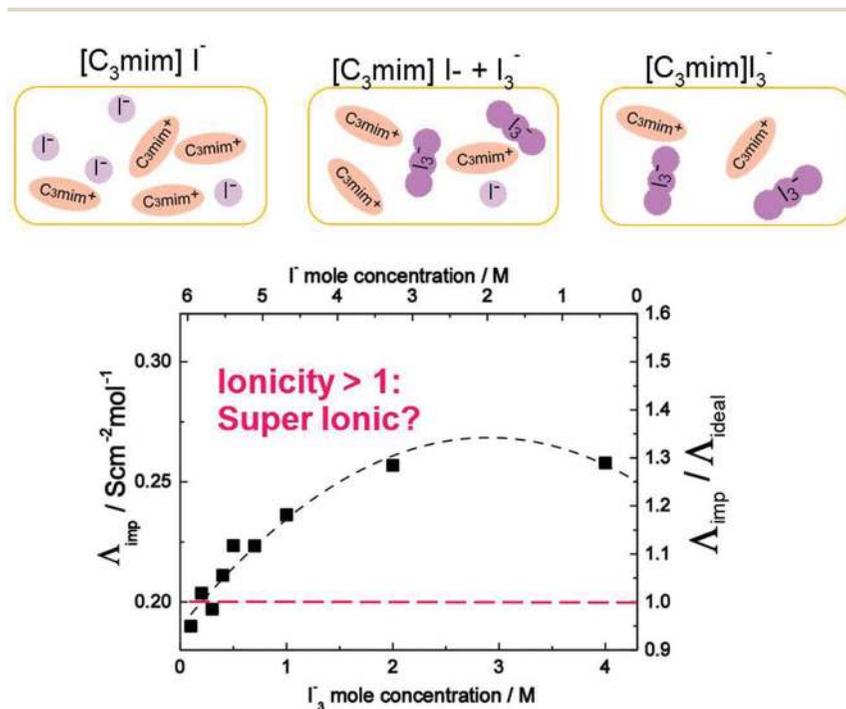

Fig. 1 Molar conductivity and $\Lambda_{imp}/\Lambda_{ideal}$ (ionicity; deviation of Walden plots from the ideal KCl line for $[C_3mim][I]/[C_3mim][I_3]$ mixtures at the same fluidity of $1/\eta = 2$ ($\eta = 0.5$ poise).

polyiodides. This is a quite significant experiment to extract the inherent and intrinsic transport process of the polyiodides.

John Slattery addressed Tamar Greaves regarding the paper by Calum J. Drummond: Earlier there was a suggestion that a procedure outlined in a recent paper by Nelyubina *et al.*[1] might be appropriate for understanding the changes in melting temperatures, clearing points and mesophase temperature ranges in your system based on anion volumes. It looks to me as though this may not be completely appropriate. Nelyubina's proposal is that clearing points and mesophase ranges (less so melting points) will decrease as the anion volume increases. This seems to be true for the clearing points, as formate has a slightly smaller volume than nitrate and the clearing points are a little higher. However, the mesophase ranges appear similar or slightly smaller based on Fig. 2 in your paper. Can you comment on this? If you analyse the data in a similar way to Nelyubina's paper do you see similar correlations? It's possible that because they derived their correlations based on some very different ionic liquid crystals that they do not hold true in the same way for your systems.

1 Y. V. Nelyubina, A. S. Shaplov, E. I. Lozinskaya, M. I. Buzin and Y. S. Vygodskii, *J. Am. Chem. Soc.*, 2016, **138**, 10076–10079.

Tamar Greaves responded: We reiterate a previous response to Markus Mezger here. In the paper by Nelyubina *et al.* 1-alkyl-3-imidazolium ionic liquids (ILs) with different anions are looked at which form liquid crystal phases. The relationship between the volumes of the cations and anion with the minimum alkyl chain length on the cation required for a liquid crystal phase to be present was investigated, along with the temperatures for melting points, clearing points and mesophase ranges. If we classify the anions purely based on their size with nitrate > formate > chloride, then we do not see any strong correlations such as those reported by Nelyubina. It is probably not valid to assume that the findings for ILs with the 1-alkyl-3-imidazolium cation will be valid for ILs containing alkylammonium cations, or more generally between aprotic and protic ILs. The interactions between the alkylammonium cation and different anions are very different compared to those for the imidazolium containing ILs. To a large extent this is due to the hydrogen bonding present for protic ILs, and arrangement of ions, such as has been discussed by Hayes *et al.*[1] Consequently, it is difficult to consider how the protic ILs can be purely described by volume.

1 R. Hayes, S. Imberti, G. G. Warr and R. Atkin, *Angew. Chem., Int. Ed.*, 2013, **52**, 4623–4627.

Jeffrey Everts continued discussion of the Agílio A. H. Pádua paper: In your paper you investigated the exfoliation of graphene using molecular dynamics where the solvent is treated explicitly. In this treatment the vacuum calculation is used as a reference state quantifying the van der Waals interaction between two graphene sheets in vacuum (including bending rigidity). However, it is well known that the dielectric spectrum of the medium affects the van der Waals interaction through the Hamaker constant within *e.g.* Landau-Lifschitz theory. Could you therefore comment or estimate how much of the PMF that you have determined in your paper is actually the result of a (reduction/enhancement) of the van der Waals interaction by changing the medium (ionic liquid (IL) instead

of vacuum), so that other effects, like specific graphene-IL interactions, can be quantified?

Agílio Pádua answered: In atomistic simulations the molecules and ions we simulate are the medium. The dielectric constant arises from dipole fluctuations in the system (we don't have explicit electronic modes unless polarisation is included explicitly in the force field). van der Waals forces (Hamaker constant) are represented by the contributions of each interaction site (atom) through its $C6/r^6$ potential. So if we change the species, the composition or the thermodynamic state (*e.g.* density) in our simulation, the properties of the "medium" reflect that automatically, within the accuracy of the interaction models.

Jeffrey Everts added: As a follow-up to my previous question, there is some temperature dependence of the Hamaker constant, because the dielectric spectra of an ionic liquid depend on the temperature. Can you therefore predict the temperature dependence of the PMF, and if so, could you tell me how you expect that it behaves? As far as I understood these are not explicitly considered in the paper.

Agílio Pádua responded: Yes. Although the pair potential is state independent, the structure and dynamics of the system depends on temperature. Ionic liquids have a wide liquid range (their gas–liquid critical point is very high) therefore modest temperature changes have little effect on thermodynamic properties like density or on structural properties. However, the effect of temperature is considerable on the transport properties.

The PMF is not a dynamic property and is related to the structure (it is the effective potential, mediated by the "solvent", that leads to the $g(r)$). The effect of temperature on $g(r)$ is expected to follow $\exp(-u/kT)$ so we think there should not be an unexpected behaviour on how the PMF depends on temperature (minima and maxima will become less pronounced at higher T).

Joshua E. S. J. Reid added to the general discussion of the paper by Calum J. Drummond: Very interesting work into the characterisation of such a variety of protic ionic liquids. You have reported various transition temperatures, such as crystal to liquid crystal. Have you considered looking at quantifying the energetics of these transitions? This would provide a deeper insight into how the variation of molecular structure impacts the interactions during these transitions. Furthermore, your use of vibrational spectroscopy of the methyl and methylene groups to define the PILs as either crystalline and liquid crystalline is very interesting. Had you considered looking at other vibrational modes that may relate to other aspects of the PILs, such as the N–H vibrations and how these change from the neat amines as a measure of extent of proton transfer, or in the case of the formate PILs, you can observe peaks corresponding to undissociated formic acid and dissociated formate anions on the basis of the C=O and COO^- asymmetric vibrations, respectively. It would be interesting to see if there was a correlation between the change in macroscopic structure with the changes in these short range, charge transfer interactions.

Tamar Greaves replied: The energetics were not quantified as part of this investigation, though we agree they would be likely to contribute interesting information. In regards to the vibrational spectra we have previously investigated the ionicity of protic ionic liquids using NMR, IR and Raman spectroscopy techniques,[1] and could identify the presence of C=O when there was excess acid in the range of 1720–1706 cm^{-1}. For these alkylammonium nitrates and formates there were no clear cases where the amine peak was present in the ionic liquids. However, there were some peaks present in the region of 3250–3400 cm^{-1} for alkylammonium nitrates with alkyl chains with 13–16 carbons present which may be possibly attributed as indicative of a small amount of free amine. There were no peaks present which could be assigned to C=O in formic acid.

1 B. Nuthakki, T. Greaves, I. Krodkiewska, A. Weerawardena, M. I. Burgar, R. J. Mulder and Calum J. Drummond, *Aust. J. Chem.*, 2007, **60**, 21–28.

Benjamin Cross directed a question to Agílio A. H. Pádua: During the peeling process, you create a cavity. This cavity is a confinement region for the ionic liquid. Do you observe a phase transition of the liquid in the confined area with graphene and/or fluorographene?

Agílio Pádua answered: In the wedge geometry of the cavity created during the peeling process the intercalation "front" of solvent ions is unidimensional. We have observed highly-ordered 2D intercalation of the first layer when performing a parallel-plate separation run. This was done in other 2D materials.[1]

1 V. Sresht, A. A. H. Pádua and D. Blankschtein, *ACS Nano*, 2015, **9**, 8255–8268.

Satyen Saha returned the discussion to the paper by Hiroshi Abe: Various polyiodide species can be generated in the system. Did you check for the presence of the $(I_4)^{2-}$? In my opinion, though it is relatively unstable, due to the uniqueness of the environment the ionic liquid creates (*i.e.* the microheterogeneity in the ionic liquid which considers the presence of both polar and nonpolar domains), this species can be stabilized. As shown in my poster, we have observed the presence of that species when investigating using multi-wavelength excitation Raman spectroscopy.

Hiroshi Abe responded: As Doctor Tsuzuki pointed out, $(I_4)^{2-}$ is unstable in the simulation box. But, it is possible for it to exist in the intermediate state between I_3^- and I_5^-. In dynamically fluctuated polyiodides, various kinds of polyiodides might be formed dynamically. At least, the population of $(I_4)^{2-}$ could be quite small, and the lifetime of $(I_4)^{2-}$ could be quite short.

Seiji Tsuzuki remarked: My comment follows on from the previous comment to Professor Abe concerning the existence of I_4^{2-}. Was the presence of other polyiodide anion species noticed? Did you observe I_4^{2-} using Raman spectra? I am not sure whether I_4^{2-} can exist or not. I think that decomposition of this molecule into I^- and I_3^- will be more stable.

Hiroshi Abe replied: Now, we are assigning polyiodides by Raman spectroscopy. It takes time to accomplish the measurements of the whole data set;

$[C_3mim][I_m]$, $[C_4mim] [I_m]$ and $[C_6mim][I_m]$. When we have finished the experiments, we will submit immediately to some journal. The existence of I_4^{2-} is significant.

Markus Mezger returned the discussion to the paper by Calum J. Drummond: Aside from the peak position that is related to the periodicity d of the total correlation function, the width of the diffuse scattering peak in the isotropic liquid that is linked to the correlation length ξ, contains important information on the remaining short-range order as shown previously.[1,2] In this context, ξ denotes the decay length of the envelope of the oscillatory correlation function rather than its periodicity d. How does the correlation length ξ in the isotropic phase depend on temperature, cation chain length, and the anion? It would be interesting to see whether connections between phase transition entropies, conformational entropies of the side chains, and the correlation lengths slightly above the LC-iso phase transition can be identified in the extensive datasets presented in your work.

1 H. Weiss, J. Mars, H. Li, G. Kircher, O. Ivanova, A. Feoktystov, O. Soltwedel, M. Bier and M. Mezger, *J. Phys. Chem. B*, 2017, **121**, 620–629.
2 D. Pontoni, J. Haddad, M. D. Michiel and M. Deutsch, *Soft Matter*, 2017, **13**, 6947–6955.

Tamar Greaves responded: This is an interesting approach to extract additional data from the pre-peak, with Weiss *et al.* and Pontoni *et al.* using a Teubner–Strey model to fit the pre-peak for their SAXS data. This model gives a correlation length and domain size. In both of the cited publications the fitted data showed that with increasing alkyl chain lengths the widths of the pre-peak became smaller, corresponding to the domain sizes increasing. Interestingly, Weiss *et al.* showed that the anion had a strong influence on the domain sizes present. Due to the relatively high clearing temperatures for these samples, there were not many samples which were isotropic liquids at 72 °C, which was the highest temperature used for the SAXS experiments. In addition, SAXS measurements were only made at 25, 50 and 72 °C. Consequently, this data is not sufficient to obtain a relationship between the isotropic phase and temperature. However, previously we have noted that the SAXS spectra at 25 or 50 °C for the shorter chained alkylammonium nitrate and formates (n less than 6) are unchanged with temperature, for those which are isotropic liquids at both temperatures.

John Slattery added: We did some work about ten years ago looking at the correlation between IL physical properties and ion volume[1] and we found that correlations were strong within specific series of ILs (*e.g.* a series of cations with a common anion), but when moving to a very different series (*e.g.* different anions) the correlations are different. It's possible that the same thing happens here, *i.e.* that the systems are quite different and so the correlations work in a different way.

1 J. M. Slattery, C. Daguenet, P. J. Dyson, T. J. S. Schubert and I. Krossing, *Angew. Chem., Int. Ed.*, 2007, **46**, 5384–5388.

John Holbrey continued: To follow on from Doctor Slattery's comments, my interpretation of the melting/clearing point phase diagrams shown in Fig. 2 of the

paper are that: the clearing points from liquid crystal to isotropic should be linked to a breakdown of intra-sheet correlations between ions in each bilayer and this should be largely independent of chain length as you observe. In contrast, the melting (crystalline solid to liquid crystal) transition includes a large component of alkyl-chain melting and, just as with hydrocarbons or alcohols *etc.*, this increases approximately monotonically with length/mass.

Tamar Greaves replied: We strongly agree. The crystalline to liquid crystal (melting) transition has a relatively linear increase with increasing alkyl chain length, which is consistent with it being predominantly due to the melting of the alkyl chains. The liquid crystal to isotropic liquid (clearing) transition is generally independent of alkyl chain length, and is consistent with disruptions to interactions between the ionic groups.

Matteo Busato returned to the general discussion of the paper by Agílio A. H. Pádua: How much of the calculated Potential of Mean Force (PMF) depends on the force field employed to simulate the ionic liquid? Is there any parameter the force field reproduces that is more important than others in simulating your system? For example, if the force field underestimates the cation and anion self-diffusion coefficients, do I get values of PMF that are reliable?

Agílio Pádua answered: As in all simulations, calculated values depend strongly on the force field used. In this application one crucial element is how to describe the interactions between the liquid and the material. We have reasonable force fields for liquids and for materials separately (reasonable here meaning we have an idea of their strengths and limitations). However, the interactions between solid materials and liquid media are much less studied. Besides even the functional forms of the interaction potentials are often not compatible (molecular force fields for organic compounds and liquids; bond order or embedded-atom potentials for materials). We are paying a lot of attention to parameterising the interactions between materials and ions or molecules directly from quantum calculations (and validating against contact angle for example) so we don't have to rely on empirical combining rules.

Simon Halstead communicated: There are various interactions which contribute to the final PMF such as strain from bending the graphene/fluorographene sheet; a loss of pi–pi interaction; a change in solvation. Would it be possible to decompose the PMF in to these contributions?

Agílio Pádua communicated in response: We decompose the contributions from bending the graphene/fluorographene sheets (including inter-layer forces) from those due to the solvent by calculating the PMF along the same route in vacuum. We can always compare two solvents without measuring the vacuum PMF, but the quantity we look at when evaluating a good/bad solvent in absolute terms is relative to the vacuum PMF.

Morgan L. Thomas communicated: For conventional lithium-ion batteries, the intercalation of lithium into graphite negative electrodes without solvent (co-)intercalation is in general desirable. However, more recently for sodium batteries

the reversible co-intercalation of Na with glyme as solvent has been reported, moreover the reversible intercalation of anions (including ILs) into graphite positive electrodes is currently being studied. In particular for the ILs it could be useful to develop some guidelines for the selection of cations/anions depending on application. In most, if not all, of the aforementioned static battery applications, exfoliation of the graphite is undesirable. Although this starkly contrasts with your detailed investigation focused on enhancing exfoliation, can you comment on the possible relevance/implications of your study? Although the use of additives in electrolytes to modify the intercalation behaviour is widely reported, considering only the pure molecular solvents or ILs, do your findings give some indication of which might be preferred?

Agílio Pádua communicated in reply: One thing that is different in battery electrodes is the electric charge on the material, which will influence both intra-layer and layer–electrolyte interactions. Nevertheless, having a good description of the fundamental interactions in the system (within the material, the liquid electrolyte, and at the interface) is useful for studying electrochemical devices. For example, the balance of forces between chelation of the metal cation with tetra-glyme or interactions with the material are crucial to understanding these solvate ionic liquids. There have been very interesting reports on the behaviour of electrolytes in nanoporous carbon materials using simulation, from which valuable insights were gained.

Christopher Hardacre opened the discussion of the paper by Rob Atkin: Do you have any thoughts about why the change in the composition of the IL systems selectively extracts different fatty acids from the algae?

Rob Atkin replied: One possible explanation for the effectiveness of [Ch][ARG] is the structure of the anion, as compared to the other IL anions. Studies have found that in applications involving ILs, the anion plays an important role in dissolving substrates, much more so than the cation.

Andrew Abbott opened a general discussion of the paper by Carin H. J. T. Dietz: You have used carboxylic acids when making up the current DES. You should consider the issue of the enthalpy of interaction between the solute and the DES. We have demonstrated that it is governed by enthalpy of hole formation due to strong interactions.[1] It is shown that the enthalpy of hole formation is lower for liquids of lower surface tension. Do you know the surface tension of your DES and would you like to comment on what effect using hydrophobic DESs will have on the enthalpy of transfer.

1 A. P. Abbott, A. Y. M. Al-Murshedi, O. A.O. Alshammari, R. C. Harris, J. H. Kareem, I. B. Qader and K. Ryder, *Fluid Phase Equil.*, 2017, **448**, 99–104.

Carin Dietz replied: As far as we know, the surface tension of hydrophobic DESs has never been measured (yet). Therefore, we are unsure about the effect of hydrophobic DESs on the enthalpy of hole formation. We fully agree that this would be a very interesting area for further exploration.

Andrew Abbott continued: Do you think the leaching of the DES from the membrane is due to the formation of surfactant structures such as micelles. Also would you like to comment on the changes in the structure of the membrane caused by the DES given the previous study on polyethylene modification using DESs?[1]

1 A. P. Abbott, T. Z. Abolibda, W. Qu, W. R. Wise and L. A. Wright, *RSC Adv.*, 2017, 7, 7268–7273.

Carin Dietz replied: We think that the main mechanism for the leaching is the low, but still measurable, water-solubility of one of the DES constituents (*i.e.*, thymol), resulting in thymol losses into the (homogeneous) aqueous phase. We did not observe any cloud or micelle formation during our experiments. Also, we did not observe any change of structure of the membrane by the DES. We think this is because we performed our experiments at room temperature, which is a too low a temperature for the reaction with PE to occur. However, when DES membranes are used for higher temperature applications, it is highly recommended to investigate the occurrence of any reaction between the DES and the membrane support.

Philip Jessop commented: Thanks for the presentation. Your process, after further development, could be very useful for separating furfural or HMF from the reaction mixture during acid-catalyzed hydrolysis of sugars or polysaccharides. That would be helpful because furfural and HMF unfortunately tend to react further to give unwanted humins. If your membrane can separate the desired products from the reaction mixture before they react further, perhaps humin formation could be avoided. However, that would expose your membrane to the acid catalyst. Would your membrane be stable or resistant to acidic solutions? Also, would protons migrate through your membrane? If protons migrate through the membrane along with HMF or furfural, then the unwanted conversion to humins might unfortunately continue.

Carin Dietz replied: We also hope that our SLMs will be suitable for *in situ* FF and HMF removal in order to prevent further undesired reactions (*e.g.* humin formation). For sure, our SLMs are able to resist acids. We namely tested the influence of pH on the DES stability, and found that acid addition did not negatively affect their stability. The only thing we have not yet studied is the migration of protons through our SLMs. This is definitely worth investigating, and it is also our intention to do this shortly because the suitability for this application is highly dependent on it. We expect that proton migration can be fully prevented using gas streams (instead of water) as the receiving phase. In any case, our SLMs are highly promising for *in situ* FF and HMF removal.

Matthieu Gras continued discussion of the paper by Rob Atkin: In this work, cheap ionic liquids are used for the extraction of lipid from algae. However, in order to perform a low cost extraction, the ionic liquid must be recovered. How is the stripping step performed? Can the recycled ionic liquid be used for several extraction cycles without losing its efficiency?

Rob Atkin answered: Recycling of the ILs is likely possible, though not investigated yet in this paper (DOI: 10.1039/c7fd00158d). We envisage addition of an alkali (*e.g.* NaOH) to neutralize the added acid followed by the removal of excess water will effect the recycling of the IL. Accumulation of biomass residuals in the ILs might occur after repeated cycles and its consequence will need to be examined.

Joshua E. S. J. Reid commented: You have shown some very nice calculations of the structures and energetic values of the ILs that you have used in aqueous solution. I wonder how well these calculations on discrete species translate to the bulk solvation structures when in the presence of water and the lipids being extracted. I would be curious to see how well these interactions translate to larger scale simulations, where you could better describe the solvation environment of the aqueous ILs with some model lipid structures.

Rob Atkin answered: We have performed neutron diffraction studies on choline lysinate, which is in some respects structurally similar to [Cho][Arg]. Preliminary fits suggest that the most likely position for the cation is the one predicted by our (gas phase) simulation. Therefore it is likely the simulation is an accurate predictor.

Philip Jessop remarked: Thanks for your presentation. How much water content was in your biomass before you performed an extraction? Drying is very energy intensive, so processes that can work on water/algae slurries are much more economical (financially and energetically) than those which require the algae to be dry.

If your process was tested on dried algae, then it would be worthwhile to try it on wet algae (*e.g.* 10 to 15 wt% algae in water). If your process still works well, then that would be particularly exciting. However, I suggest that you should test algae that has never been dried. Some researchers simply add water to dried algae and use that as a sample of "wet algae", but that is not scientifically valid. Because the drying process usually causes cell rupture, such a "wet algae" sample would be artificially easy to extract. A more scientifically valid test would be to test your process on algae that has never been dried.

Rob Atkin answered: We agree with this comment and plan to do this next.

Doug MacFarlane questioned: Once the proton is removed from the amino acid you have an anion which is quite basic. Presumably the primary mode of action of the IL is attacking the cellular structure and releasing the membrane lipids and the basicity might be helpful in this regard. But is it going to deprotonate other groups and become less effective as the reaction proceeds?

Rob Atkin responded: Good suggestion. I think if we were to examine different yields at different reaction times this could provide some insight.

José Nuno Canongia Lopes queried: What about other amino acids? Is this interplay between intra- and intermolecular interactions that explain the

extraction enhancement also possible in other amino acids? (Not necessarily natural amino acids). This would be a good test to the assumption being forwarded.

Rob Atkin answered: Arginate is the only AA with a guanidinium group. If our calculations are correct then arginate is the only AA capable of producing such strong H-bonds. Synthetic AA containing a guanidinium group are therefore of interest.

Tristan Youngs commented: After discussion of the new results presented by Professor Atkin, and questioning the role of the intramolecular hydrogen bond in the bare cation, the correlation between the presence of said motif in both arginate and lysinate and the enhanced efficiency for dissolution of the material in these ILs is clear. However, it remains to be seen whether this correlation is circumstantial, or the structural motif bears some relevance to the dissolution mechanism.

José Nuno Canongia Lopes asked: My question is about the exfoliation of graphite sheets with ILs that contain aromatic rings (benzene) in their structure. The basic assumption is that the oblate shape of benzene helps it to insert itself between the sheets (in an horizontal position) and enhance the exfoliation process. However, due to its strong quadrupole moment I think that a benzene moiety would probably interact with the sheets along its long axes (vertical position). Is there any proof that is could enter horizontally and then twist to a vertical position?

Rob Atkin replied: Our AFM images show that certain C2 imidazolium cations (with anions that give surface energies matching graphene) can affect spontaneous exfoliation of graphene from HOPG. The process is not fast, and (depending on the IL) requires something like 60–120 min before commencing at room temperature. The time to first exfoliation decreases with T in a manner consistent with first order kinetics. We have tested ionic liquids with the "correct" surface tension but non-aromatic cations, and spontaneous exfoliation is not observed.

Therefore, the evidence that the cation inserts between the sheets is that spontaneous exfoliation is observed. The fact that it takes some time to begin is consistent with the cation not preferentially having an orientation for insertion, but sometimes it will, and can insert. Our images suggest that once exfoliation begins, it cascades, which is consistent with it adopting a vertical orientation between sheets, and thereby eventually effecting exfoliation.

Magdalena Bendová returned the discussion to the paper by Carin H. J. T. Dietz: Did you also study the membrane selectivity, since a good permeability is of course essential, but only one part of the equation? Also what is the economic viability of the DES used in your study, it does not seem to be the most cost-effective? Does a little go a long way? For instance, lidocaine may not be the cheapest compound to use in a membrane.

Carin Dietz answered: It is true that we only studied the membrane selectivities, but not the economics (yet). However, we think our DESs will probably be economically viable. The reason is that the cost price of one of the best performing DES (deca-thy) is comparable to that of toluene, but since a distillation step to purify the solvent is no longer required in the newly developed process using an SLM, the energy costs will be significantly lower. Also, the amount of DES required is much lower than the amount of toluene needed in a conventional extraction process.

John Holbrey addressed Rob Atkin and Carin H. J. T. Dietz: Have you considered the importance (or potential) for cooperative effects between the ionic liquid/DES components in driving your extraction processes, by exploring temperature effects? My thought is that, if ion-pair association is critical to synergistic extraction (if that isn't going too far) then increasing the temperature might reduce ion-pair association and reduce extraction.

Rob Atkin replied: Excellent suggestion, this is something that will be investigated.

Carin Dietz responded: Since most of our hydrophobic DESs do not contain salts, there is no ion pair association involved in the extraction process. We did investigate the influence of temperature on the FF and HMF removal (at 30, 50 and 80 °C). We actually found a slight increase in extraction performance at higher temperatures (probably due to lower mass transfer limitations as a result of the lower viscosity of the DES at higher temperatures), but this effect was only very minor.

Johan Jacquemin continued the discussion of the paper by Carin H. J. T. Dietz: According to Table 7 of your paper (DOI: 10.1039/c7fd00152e), the weight-loss of Br after 24 h of diffusion is very large, did you investigate the composition changes in the liquid membrane and in the liquid passing through this membrane? The 50% bromide weight-loss must have gone somewhere. Furthermore, did you try to understand the diffusion mechanism of the liquid passing through each membrane? It is highly important to know the different effects on the mechanism to determine then the correct mechanism pathway.

Carin Dietz replied: We fully agree that it is interesting to know where the bromide losses have gone too. We expect it is lost to the water phase by dissolution, but since the bromide losses are very small, it will be impossible to measure them (below the detection limit). Moreover, we would like to stress that the 50% loss was only found at the surface, as EDX is a surface-measurement technique. This means that the overall bromide losses are far less than this number, as most of the DES is not located at the surface but in the pores.

Margarida Costa Gomes asked: Could you explain what are the advantages of using a membrane separation technique for solvents that are not miscible with water? Does this technique perform better than liquid–liquid extraction?

Carin Dietz answered: The main advantages of membrane separation over liquid–liquid extraction are: (i) the lower amount of solvent required (lower solvent-to-feed ratio), (ii) the larger contact area (smaller equipment), (iii) the easier solvent recovery, and (iv) the lower cost.

Margarida Costa Gomes queried: How easy would it be to find other DESs with better performances?

Carin Dietz answered: So far, the DESs have not been optimized for this specific application. We have just tested 4 different hydrophobic DESs. We think a combination of experimental trial-and-error with more sophisticated theoretical models predicting the physicochemical properties of DESs is required in order to find an optimized solvent for each application. This will still require a lot of research.

Margarida Costa Gomes questioned: What is the value of the extraction coefficient that would be required to proceed for a scale-up of the membrane separation process using the DES you study?

Carin Dietz replied: At this moment, the membrane separation process has not been scaled up. However, within our project a pilot-scale set-up is expected to be built within the next two years, which will allow a better evaluation of the scaled up process. So far, the DESs have not been optimized for this specific application. We just tested 4 different hydrophobic DESs. We think a combination of experimental trial-and-error with more sophisticated theoretical models predicting the physicochemical properties of DESs is required in order to find an optimized solvent for each application. This will still require a lot of research.

Leigh Aldous addressed Rob Atkin: You observed different responses for different anions. Do you think the effect is primarily kinetic or thermodynamic in nature?

Rob Atkin responded: As we only studied one temperature for one time length, we cannot comment on this with certainty. However, our calculations for [Cho][Arg] lead us to suspect a thermodynamic role. Different reaction conditions would need to be tested to resolve the kinetic issue.

Christopher Hardacre continued discussion of the paper by Carin H. J. T. Dietz: Can you clarify how you undertook the stability measurements of the SLM in air? A large decrease is observed and yet the components have relatively low volatility at room temperature. Can you explain what is being lost?

Carin Dietz replied: SLM stability measurements were undertaken by storing the membrane in open air and measuring its weight loss after 24 h. We think that the weight loss is due to the evaporation of the DES's constituents with the highest volatility, *i.e.* thymol and menthol. We also noticed that a larger pore size of the membrane resulted in a larger weight loss due to an increased contact with the air. The lowest weight loss was observed using the SLM consisting of

deca-n8888Br on PP2E HF (with the lowest volatility of both DES's constituents and the smallest pore size), this was only 2 wt%.

Christopher Hardacre asked: In order to study the mechanism of the air stability of the SLMs and their mass loss, is it worth extracting the SLMs following exposure in air to examine the components left in the system?

Carin Dietz replied: We fully agree that the analysis of the DES after air exposure is interesting for air stability measurements. As we were applying the SLMs for extraction of FF and HMF from the aqueous phases, we think that the analysis of the DES in the membrane after water exposure is even more interesting for our application. As far as we know, this has not yet been studied.

Margarida Costa Gomes asked: Are you sure that no chemical reactions take place between the species in solution and the constituents of your membrane? Have you analyzed the species present in your system during the separation?

Carin Dietz answered: For all twelve SLM systems, we analyzed the composition of the water phase after the membrane separation at room temperature. In all cases, we only found FF, HMF and traces of thymol (in the case a DES containing thymol was used) in the water phase, but did not detect any other DES constituents or other side products. For two hydrophobic DESs (deca-thy and thy-lid) originating from the SLMs after membrane separation at room temperature, we have also studied the composition and only found minor amounts of FF and HMF and no signs of other side products. Therefore, we think that there are no chemical reactions occurring between the FF and HMF and the membrane constituents. Most probably, the temperature (25 °C) is too low for any side reactions to occur.

Philip Jessop addressed Rob Atkin and Carin H. J. T. Dietz: When working towards improving your systems, I recommend that you consider the cost of your IL or DES and how that cost compares to the product you are producing. This will allow you to estimate how much IL you can afford to lose per cycle before it is no longer economically viable. For example, we can assume that the cost of replacing lost IL must not be greater than 10% of the cost of the product you're making. (Probably the % is lower than that, but let's stick with that number). Find out how much one batch of product is worth. Calculate 10% of that value. That's how much IL (in euro, for example) you can afford to lose. If you can estimate the value of the IL (in euro per g) then you can convert this to how many g of IL you can afford to lose per cycle. This is a simple calculation but well worthwhile because it puts the process in perspective. If the process isn't economical yet but is close enough that it might be economical after optimization, then great! If, after the calculation, you find out that the process has no hope of ever being economical, then that's worth knowing too. In that case, the research might still be fine fundamental research but won't make a viable process in the near term. Maybe the process could be modified so it would produce a more valuable product or use a less valuable IL?

Rob Atkin replied: I agree with some of what you say, but not completely. In my opinion, placing an economic constraint on the research at such an early stage runs the risk of stifling creativity. At this point I think it is valid to pursue all avenues, as we cannot know what discoveries might be made by other groups to render a process economically viable. This research is mostly of a fundamental nature; it has an applied goal but I am interested in the work from a fundamental perspective, and the results obtained here are already informing other areas of our research.

Carin Dietz added: We fully agree that this should be thoroughly calculated and reported. We would already like to mention that our DES losses have decreased recently, since we are using gas streams (instead of water) as the receiving phase. That would make the economics more favorable.

Doug MacFarlane addressed Rob Atkin: The idea of designer amino acids is interesting. Could you speculate on what they might look like and where the amino and acid functional groups might ideally be in the molecule.

Rob Atkin responded: Our calculations suggest a need to maintain the close association of the cation charge group with the guanidinium group. However, adding a hydrophobic moiety to either the cation or anion to introduce amphiphilic character may aid dissolution.

Joshua E. S. J. Reid continued discussion of the paper by Carin H. J. T. Dietz: I am curious about what has happened with the DES after the tests for stability. I would be curious to know what has been lost, but more importantly what has remained? Is the remaining composition of the DES components a more stable DES to be used in these membranes? If not strictly speaking a DES, it may be less important if the chemistry works just as well if not better.

Carin Dietz answered: We fully agree. We are most interested in the analysis of the DES in the membrane after water exposure. As far as we know, this has not yet been studied.

Florian Maier addressed Rob Atkin: Regarding the impact of the salinity effects of your IL solution on the cells employed in your study. Making a rough estimation of your concentration used, *e.g.* 60 wt% of *e.g.* choline glycinate in water, corresponds roughly to a concentration of 4 mol l^{-1} salt solution (analogue for NaCl, 4 mol l^{-1}) it is not very far from saturation solubility. Hence, I was wondering if the observed impact in cell pre-treatment employing the IL solution (that is, denaturation of proteins, cell compartment decomposition, membranes, cell walls *etc.*) might be related to the high salt concentration (osmosis effects). It has been shown in a recent work that 0.2 mol l^{-1} NaCl and KCl solutions have a considerable impact on the algae cells within a couple of hours. The impact of the different salts (including an iso-osmotic non-ionic solution) is rather complicated as described by the authors.[1] Nevertheless, in your study, control experiments might be helpful for understanding the extraction process. In a future study, it would be interesting to demonstrate the changes in the cells (*e.g.* light microscopy *etc.*).

1 M. J. Affenzeller, A. Darehshouri, A. Andosch, C. Lütz and U. Lütz-Meindl, *J. Exp. Bot.*, 2009, **60**, 939–954.

Philip Jessop commented: It might be interesting to look at whether your IL pretreatment causes cell rupturing. If you use microscopy or Nile Red dye plus microscopy, could you compare the cells before and after the IL pretreatment to see if cell wall rupturing has occurred?

Rob Atkin replied: Thank you for this suggestion. Yes, this is an experiment we will do.

Himani Medhi communicated to Carin H. J. T. Dietz: You have concluded that "Higher diffusion was also achieved using DESs with lower viscosity and stronger interactions with FF and HMF". What was the experimental evidence and or theoretical method that supported the above statement?

Carin Dietz communicated in reply: It was experimentally observed that DES deca-N8888Br has the highest viscosity of all the tested DESs, and also the lowest permeation for FF and HMF (probably due to mass transfer limitations). The other three DESs had very similar viscosities, but we still observed a large difference in their permeation performance. This could then only be attributed to the differences in their interactions with FF and HMF. Therefore, we concluded that higher diffusivities were obtained with DESs with lower viscosities and stronger interactions with FF and HMF.