Future hopes for ground-based gravitational-wave astronomy

Ground-based gravitational wave detectors are being improved all the time. This will lead to substantially enhanced observations using the current infrastructure, and it is hoped that in the 2030s and 2040s the next generation of instruments (the Einstein Telescope and the Cosmic Explorer, which we will call ET and CE) will improve sensitivities by a factor of 10 to 20. What can we expect, and what can we hope for, from these future observations?

More of the same, but better

The safest bet is that observations over longer times with more sensitive instruments will yield more events. Recall that the strain amplitude scales like $1/r$ for a distance r, which means that (for example) if the sensitivity is doubled, and if the horizon to detectable events is close enough that the redshift is not large (which is true at the moment), then the accessible volume increases by a factor of 8. It seems plausible that the number of events per time was larger in the past (e.g., because the star formation rate was greater), which means that increased sensitivity might lead to an even greater number of binary coalescences.

As a result, if there are bumps or other characteristic features in the distribution of black hole masses, mass ratios, spin parameters, orientations, etc., they are guaranteed to become more statistically significant than they are currently. This might provide enough information to make it more difficult to fit models. This will be good news, because with greater constraints will come better understanding of black hole binary formation within a given channel, and possibly even a decision about which channel explains the observations best. Extra depth to the observations will be reached when we have so many events that we can break them down into slices of luminosity distance and thus see the evolution of the events with cosmic time.

For neutron star binaries, currently we have very few (just two that are probable NS-NS binaries, and a few that are NS-BH binaries). With such a small number we do not know whether our sample is representative or unusual. Increasing the sample by a factor of 10, which is plausible within the next two observing runs (O4 and O5), will push such detections into the realm of statistics rather than the current realm of happenstance. In the third generation (of ET and CE), the rates will increase by a factor of ~ 1000 or possibly more, and then an additional challenge will be to streamline analysis pipelines.

In addition to the increased rate of events, better sensitivity will mean that events just like those that have already been seen will be observed with greater signal to noise. This means that those events, which will constitute the strongest signals of a larger set, will be measured more precisely than they are currently. Some aspects of current observations, such as the chirp mass of a double neutron star merger, are already measured so precisely (fractional precision $\sim 10^{-3}$!) that it's not clear that a better measurement will be meaningful. But many black hole events have worse precision, so improvement will help. Moreover, with larger signal to noise we will be able to measure higher-order effects. For example, we noted that the mass ratio affects the waveform but only subtlely. With stronger events and thus more precise measurements of the mass ratio as well as the chirp mass, both masses in a binary can be obtained. Similarly, the spins of the black holes thus far detected seem typically to be small. Better measurements will determine whether they appear that way to greater precision, or whether there really is a significant slant toward spins aligned with the orbital axis rather than counteraligned.

As the final entry in this category, we have already indicated that among black holes, low-mass black holes give weaker signals than high-mass black holes. Better sensitivity and longer observations will tell us whether there is a major deficit of low-mass black holes in the intrinsic population; pretty decent indications exist, but more data will be important. Note that this need not demonstrate that when black holes are *born* there are few at low masses, just that such black holes aren't commonly in double black hole systems that merge rapidly enough to be seen.

Weird individual events

When more things can be seen, long astronomical experience indicates that in addition to improving statistics for whatever we might consider "ordinary", there will be some extraordinary events that can teach us a lot because of their weirdness. In a rough sense, the ~ 100 events seen currently have given us some that are "one in a hundred" occurrences. We'll see ten of those in 1000 events, along with possibly more important "one in a thousand" systems. Almost by definition these are difficult to predict, but here is a wish list:

1. A massive black hole binary in which at least one component is definitely in the pair instability mass gap. There are some candidates now, but the uncertainty in the masses of the black holes is enough that you might be able to get away by claiming that one hole is below the mass gap and one is above. But with better precision, you might find some black hole with a mass of $85 \pm 5\ M_\odot$, and then standard isolated binary formation can't do it. Previous history suggests that binary population modelers could find a clever way to do this on rare occasions, but at least we could make them squirm!

2. A massive black hole binary in which at least one component is clearly *above* the pair instability mass gap. Suppose we believe that there is a mass gap. There is already a decreasing number of black holes going toward the mass gap (around $40 - 60\ M_\odot$). Maybe we just run out of stars which can produce black holes that massive. But if we see some below the gap, none definitely in the gap, and some *above* the gap, then the

gap really looks like a gap. We would then give the binary population modelers their due credit.

3. A definitely eccentric binary black hole merger, say $e > 0.2$ at a frequency > 50 Hz. In isolated binary evolution, the black holes are expected to start out their binary lives in an orbit which is not far from circular. Since gravitational radiation circularizes binaries, this means that via this channel we would think that any merger would be quasi-circular (quasi, because the orbits inspiral). In contrast, dynamical channels have ways to get palpable eccentricity at tens of Hertz. For example, some fraction of binary-single interactions involving black holes will lead to a BH-BH system with a very small pericenter. This isn't probable, but it can happen. Thus if a clear eccentricity is seen, that will point toward a dynamical origin (although as always we shouldn't underestimate the cleverness of binary modelers!). We want our event to show clear eccentricity > 50 Hz because at lower frequencies the detectors aren't as sensitive and thus waveforms can't easily distinguish between eccentricity and other effects such as precession. Which brings us to...

4. Clear precession in a binary. If the rotational angular momentum of either member of a binary is misaligned with the orbital angular momentum, then the orbit can precess. This is easiest if one of the binary components is much more massive than the other, e.g., in a BH-NS system, because then the rotational angular momentum can even exceed the orbital angular momentum. Such misalignment might be explained in various ways (dynamical processes are an easy scenario), but this would also put models to a stress test.

5. NS-NS binaries of a wide range of masses. Known NS-NS binaries in the Galaxy have a pretty tight range of masses. The first two probably NS-NS events seen, however, might indicate a wider range. Is this typical? If the few NS-NS coalescences seen in O4 and O5 have a broad set of masses, this might indicate that processes not typical of our Galaxy are applying. By the time of ET and CE, we should have an excellent census.

6. Something that clearly violates classical general relativity. Okay, that isn't likely, but people have proposed potential signatures including "echoes" of a gravitational waveform after a merger. At the very least, we can be assured that as observations become more numerous and better, either GR will be confirmed more and more in strong gravity environments, or if there is a clear deviation then many theorists will have use for their time!

Nuclear physics and cosmology

To me, one of the most interesting guaranteed physics returns from future gravitational wave detections is what we will learn about the properties of the dense matter in the cores of neutron stars. Briefly, this matter is (1) up to a few times denser than atomic nuclei, (2) very neutron rich, with a neutron to proton ratio of $10 - 100$, depending on the density, and (3) "cold", in the sense that the temperature is much less than the Fermi temperature, and thus the matter is highly degenerate (unlike, say, the matter in heavy ion collisions, which can reach comparable total energy densities to the densities in neutron star cores). This realm of matter cannot be explored in laboratories. Moreover, the fundamental physical theory of the strong interaction, quantum chromodynamics, cannot be used to provide first-principles calculations because of an awful computational problem called the "fermion sign problem". This means that observations of neutron stars are the only way to make progress. In a broad sense, because the properties of neutron star core matter affect macroscopic observables (such as the radius as a function of mass, the maximum mass, the tidal deformability, and the moment of inertia), macroscopic observations paired with proper Bayesian statistical analysis can constrain the properties of cold matter at high densities.

Electromagnetic observations have helped. A lot. Observations and precise measurements of pulsars have uncovered a few with high masses (the current record-holder for a precisely-measured mass is $\sim 2.1\ M_\odot$). X-ray observations with NASA's Neutron star Interior Composition Explorer (NICER) have provided reliable and reasonably precise radii for two pulsars, and more will follow with time. Although it has not yet come to pass, it is expected that within a decade the moment of inertia of a pulsar in a double pulsar system will be measured.

In the long term, however, it seems inevitable to me that gravitational waves will take over, particularly for neutron stars with masses in the vicinity of $1.4\ M_\odot$. The GW170817 event, despite its relatively high signal to noise ratio (of about 30), provided only an upper limit to the tidal deformability and thus in effect only an upper limit to the radius. But with time we will see more events and that increased number, plus a few events which have larger signal to noise, should improve the precision with which the tidal deformability is known. By the time of ET and CE, there will be some events that could be so strong that not just the lowest-order tidal deformability, but also oscillations induced near merger, could be seen. It will eventually even be possible to see *post*-merger oscillations, which will be sensitive to the properties of somewhat hot dense matter.

NICER may still be the last word for a while for high-mass neutron stars (the team has reported results on the $2.1\ M_\odot$ pulsar), because the tidal deformability scales with the neutron star mass M something like M^{-6}. It is also not clear that such heavy neutron stars will be found in the double neutron star systems that could be seen using gravitational waves.

But it seems certain that nuclear physics will benefit tremendously from future gravitational wave observations.

Our final topic is cosmology, in particular measurements of the Hubble parameter using gravitational waves. Hubble's Law is that the expansion of the universe imprints an apparent recession speed v of an object (e.g., a galaxy) which is linearly proportional to its distance d from us: $v = H_0 d$. This is sometimes called the Hubble "constant" because at a given time after the Big Bang every observer should measure the same H_0. But H_0 isn't constant in time; indeed, the nature of its variation with cosmic time is fundamentally what led to the discovery that the universe is accelerating in its expansion.

To measure H_0, you need to be able to measure d and v. In electromagnetic observations, v is "easy": just figure out the redshift of the host galaxy of whatever you're observing, and that gives you v. Electromagnetically, d is hard: you need to use a "standard candle", which has a known luminosity L, and see what flux F you receive, and then the distance is $d = \sqrt{L/(4\pi F)}$. There are lots of potential systematic errors in the determination of d, but major efforts over decades have reduced them significantly.

The attraction of gravitational wave astronomy in this respect is that it flips the script: assuming general relativity is correct, observation of (say) a double black hole merger gives you the luminosity distance directly (although there is ambiguity related to the inclination of the binary with respect to us). However, the poor localization of such sources (such that thousands of galaxies will be in the error volumes) means that we can't say much about the redshift.

We might be able to get around this statistically; with enough events, and given that H_0 has some value, it has been proposed that it will be possible to map candidate host galaxies event by event and solve for H_0. This will take a while, and will be additionally challenging because deep electromagnetic observations will be needed to map out possible galactic hosts. With thousands of events, will telescope allocation committees give the time? Also, kicks mean that especially low-mass compact binaries might not be obviously associated with particular galaxies.

If an event involves a neutron star, and if there is thus an electromagnetic counterpart that can be identified clearly, then that makes things easier because now we have both the distance and the redshift. Such events would still have a potential ambiguity based on the orientation of the binary to us (which affects the inference of the distance), but in bulk they would provide a way to determine H_0 that is completely independent of current electromagnetic methods.

Currently there is special interest in H_0 because various methods appear to disagree by an amount that a few times larger than their estimated uncertainties. Could this indicate the presence of new physics? Maybe, but mutually incompatible measurements of H_0 have existed before, and always the resolution was underestimated systematic errors. But the tantalizing possibility of something dramatic has ramped up measurements of H_0 in many ways.

Could gravitational wave measurements of H_0 play an important role? Yes, it's definitely possible. Some predictions are that within tens of NS-NS detections, H_0 will be known to 1% by this method alone, and that this could prove decisive in resolving the actual value and determining whether new physics is needed. I'm a bit more cautious about the prospects than some people, for two reasons: (1) systematic effects have always played a significant role, and we are assuming that there are no such effects in gravitational wave observations at the 1% level, (2) efforts at electromagnetic inference of H_0 are accelerating, with JWST and other observations, which means that by the time that gravitational wave observations get to 1% (if they do), the bar will have moved. But there is no question that this is an important driver of electromagnetic follow-up of gravitational-wave-detected double neutron star coalescences. We can certainly hope!

What excites you most about future gravitational wave observations?