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Knowledge about others reduces one's own sense of anonymity

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Social ties often seem symmetric, but they need not be[1–5]. For example, a person might know a stranger better than the stranger knows them. We explored whether people overlook these asymmetries and what consequences that might have for people's perceptions and actions. Here we show that when people know more about others, they think others know more about them. Across nine laboratory experiments, when participants learned more about a stranger, they felt as if the stranger also knew them better, and they acted as if the stranger was more attuned to their actions. As a result, participants were more honest around known strangers. We tested this further with a field experiment in New York City, in which we provided residents with mundane information about neighbourhood police officers. We found that the intervention shifted residents' perceptions of officers' knowledge of illegal activity, and it may even have reduced crime. It appears that our sense of anonymity depends not only on what people know about us but also on what we know about them.

People often assume that social ties are symmetric[1–3]. For instance, people are faster to recognize symmetric social ties[4]. People also mentally fill in gaps that make social ties seem more symmetric than they are[5]. This symmetry assumption often makes sense because our social ties are usually reciprocated. However, they need not be. Although people can recognize that asymmetric ties exist, doing so requires greater cognitive effort[1]. We can perhaps think of the symmetry assumption as a common social heuristic[6].

Here we explore how overgeneralizing this assumption affects people's sense of anonymity. For example, suppose we learn a few details about someone who is otherwise a stranger. In reality, knowing more about this person does not affect what they know about us. However, psychologically, people's sense of being understood often correlates with their sense of how much they understand others[7]. People also often anchor on their own perspective when making inferences about others[8,9]. Therefore, learning more about others might lead us to believe they know more about us too. That is, we might experience a greater illusion of transparency—or the sense that our thoughts and actions are more obvious to others than they actually are[10,11].

This can shift the fundamental nature of how we perceive and act around others. For instance, although the typical experience with strangers is one of relative anonymity[12], when people overlook asymmetries in their social ties, they may feel less anonymous around 'known strangers' (that is, strangers whom they have information about). This might not only affect people's perceptions of what others know but also people's behaviour around known strangers. Previous research has shown how anonymity can increase dishonest or harmful behaviour, while reducing cooperative or prosocial behaviour[13–17]. However, if people believe that known strangers know more about them, and are more attuned to their actions, then this might reduce some of the negative behavioural consequences of anonymity. In the company of known strangers, people may be more honest or less likely to cheat.

We report nine laboratory experiments that tested whether people feel (and act as if they are) less anonymous when interacting with known strangers than with unknown strangers. We found that people think known strangers know or understand them better. People also believe that known strangers are better at detecting their lies. Furthermore, people are more honest on a task when known strangers are responsible for catching cheating behaviour.

We then describe a field experiment with the New York City Police Department (NYPD) and the New York City Housing Authority (NYCHA), exploring how providing residents with information about neighbourhood officers affects (1) residents' beliefs about officer knowledge and (2) crime. We found that residents in developments who received this information thought that neighbourhood officers were more likely to know whether they did something illegal, and we found suggestive evidence that the intervention reduced reported crimes.

Laboratory experiments

The laboratory experiments all followed the same general paradigm (see the Methods section for full details). Participants (recruited from Amazon Mechanical Turk) were told that they would be interacting online with another person, and although it would mostly be anonymous, they might be asked to share some information by answering simple 'icebreaker' questions. Deception was involved: participants did not actually interact with another person. 'Partner' responses were pre-programmed. Across all studies, the independent variable was whether participants were shown their partners' responses to the icebreaker questions. Participants were told that their assigned experimental condition determined whether they saw their partners' responses.

We used three experimental paradigms, with replications and extensions within each. For each, we lead with the final experiment conducted because it included checks for suspicion about the deception.

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Table 1 | Primary results for laboratory experiments

Experiment	Outcome	Results	Test statistic	*P* value	Effect size
Beliefs about known strangers' knowledge					
1A	Stranger knowledge	M_{info}=5.22 (4.86–5.59), n=206; $M_{no\ info}$=4.53 (4.12–4.93), n=191	$t(395)$ =2.50	0.01	d=0.25
1B	Stranger knowledge	M_{info}=4.06 (3.72–4.41), n=142; $M_{no\ info}$=3.55 (3.23–3.87), n=149	$t(289)$ =2.14	0.03	d=0.25
1C	Stranger knowledge	$M_{similar}$=3.53 (3.25–3.82), n=156; $M_{dissimilar}$=3.68 (3.38–3.98), n=149; $M_{no\ info}$=3.22 (2.94–3.50), n=151	$t(453)$ =2.13	0.03	d=0.21
1D	Stranger knowledge	M_{info}=3.58 (3.37–3.80), n=279; $M_{no\ info}$=3.22 (2.99–3.45), n=264	$t(541)$ =2.23	0.03	d=0.19
Perceptions of lie detection					
2A	Stranger knowledge	M_{info}=41.06% (37.76–44.35%), n=228; $M_{no\ info}$=33.29% (30.34–36.24%), n=234	$t(453.20)$ =3.44	<0.001	d=0.32
2B	Stranger knowledge	$M_{4\ facts}$=36.85% (33.40–40.29%), n=181; $M_{1\ fact}$=38.04% (34.28–41.80%), n=184; $M_{no\ info}$=30.94% (28.12–33.75%), n=187	$F(2, 549)$=4.98	0.007	η_p^2=0.02
Honesty around known strangers					
3A	Stranger knowledge	M_{info}=4.32 (4.15–4.49), n=494; $M_{no\ info}$=3.94 (3.76–4.13), n=508	$t(994.87)$ =2.96	0.003	d=0.19
3A	Honesty	M_{info}=41.6%, n=490; $M_{no\ info}$=49.3%, n=505	χ^2=5.91	0.02	V=0.08
3B	Stranger knowledge	M_{info}=3.37 (3.15–3.59), n=284; $M_{no\ info}$=2.99 (2.76–3.21), n=298	$t(580)$=2.40	0.02	d=0.20
3B	Honesty	M_{info}=55.6%, n=284; $M_{no\ info}$=64.4%, n=298	χ^2=4.69	0.03	V=0.09
3C	Honesty	M_{info}=41.3%, n=143; $M_{no\ info}$=55.6%, n=151	χ^2=6.07	0.01	V=0.14

When participants had information about their partners, they believed their partners knew them better and would be better at detecting their lies, and participants behaved more honestly. 'Stranger knowledge' refers to participants' judgments of how well their partners knew them (measured with a ten-point scale for experiments 1A–1C, a seven-point scale for experiments 1D and 3A–3B, and a per cent likelihood that their partners would detect their lie in experiments 2A–2B; 95% confidence intervals are shown in parentheses). 'Honesty' refers to the rate at which participants reported having had surgery, which they may or may not have had. *P* values are from two-tailed tests.

Beliefs about the knowledge of known strangers

In experiment 1A (n = 402), participants first completed an icebreaker in which they were asked three multiple choice questions about the type of area they live in, their marital and family status, and their work status. We selected these questions because they are generic. Knowing a person's answers to them is minimally informative. Someone's answers might be enough to distinguish them from a perfect stranger, but hardly enough to build much of a social connection. Participants were randomly assigned to one of two conditions. In the 'information' condition, participants were shown their partner's responses (which were randomly generated); in the 'no information' condition, participants did not see these responses. Participants were then told that their marital and family status had been shared with their partner, and their partner was trying to guess their answers to the other two icebreaker questions. While their partner was supposedly making their guesses, participants were asked to rate how much they felt that their partners knew them on a ten-point scale (in which higher numbers indicate more knowledge, adapted from ref. [18]; five participants were excluded for missing responses).

Participants in the information condition believed that their partners knew them better (information: M = 5.22, 95% CI = 4.86–5.59; versus no information: M = 4.53, 95% CI = 4.12–4.93, $t(395)$ = 2.50, P = 0.01, d = 0.25). Before answering questions on demographics, participants were asked whether they believed that they were connected to another person (one participant was excluded for a missing response). Suspicion did not significantly differ across conditions (71.7% of participants believed that they were connected in the information condition versus 69.4% in the no information condition, χ^2 = 0.26, P = 0.61; using TOST[19], we can reject a difference of more than 10%, Z = 1.69, P < 0.05).

The procedure in experiment 1B (n = 291) was identical, but without the suspicion check, and we found the same results (for detailed results, see Table 1; $t(289)$ = 2.14, P = 0.03, d = 0.25). Experiment 1C (n = 457; one participant was excluded for a missing response) tested whether these findings were driven by perceived similarity to one's partner, by including three conditions: no information, dissimilar partner (in which no pieces of partner information matched the responses of the participant) and similar partner (in which two pieces of partner information matched the responses of the participant). Again, the findings were replicated ($t(453)$ = 2.13, P = 0.03, d = 0.21), and the effect appears to be driven simply by whether participants have information about their partner, not by the similarity of the partner. Experiment 1D (n = 548; five participants were excluded for missing responses) replicated our initial findings in a paradigm in which participants did not share any information about themselves ($t(541)$ = 2.23, P = 0.03, d = 0.19).

Together, these studies offer initial support for the hypothesis that having more information about others leads people to believe that others know them better too. Next, we tested whether this subjective feeling shapes people's perceptions of how attuned known strangers are to specific actions.

Perceptions of lie detection

In experiment 2A (n = 462), participants first completed an icebreaker in which they wrote down four truths and one lie about themselves. Participants were randomly assigned to one of two conditions. In the no information condition, participants were not shown any statements about their partner. In the information condition, participants were shown the four truths that their partner supposedly wrote. The partner's responses were randomly drawn from preprogrammed statements, which were designed to be fairly mundane.

Participants were then asked how likely (as a percentage) they thought it was that their partner would guess which of their five statements was a lie. Participants then indicated how well they felt they knew their partner (on a seven-point scale) and whether they believed they were connected to another person.

Participants in the information condition believed that there was a higher likelihood that their partner would detect their lie (information: $M = 41.06\%$, 95% CI = 37.76–44.35%; versus no information: $M = 33.29\%$, 95% CI = 30.34–36.24%), independent samples t-test for unequal variances: $t(453.20) = 3.44$, $P < 0.001$, $d = 0.32$). Moreover, participants in the information condition felt as if they knew their partner better (information: $M = 3.04$, 95% CI = 2.83–3.25; versus no information: $M = 1.89$, 95% CI = 1.69–2.09), $t(460) = 7.73$, $P < 0.001$, $d = 0.72$). In a mediation analysis[20], we found that participants' feelings of knowing their partner significantly mediated the effect of information on participants' beliefs that their partners would detect their lies (indirect effect = 3.83, bias-corrected 95% CI = 1.91–5.99). In this paradigm, participants in the information condition were more likely to believe that they were connected to another person (58.3% (information condition) versus 40.6% (no information condition), $\chi^2 = 14.53$, $P < 0.001$, Cramer's $V = 0.18$). The results hold when we restrict the analysis to participants who believed that they were connected to another person (Supplementary Information A.2).

Experiment 2B ($n = 552$) had a similar design, but with three conditions: no information, one fact (participants were shown only one of their partner's truths) and four facts (identical to the information condition above). Participants were not asked how well they knew their partner or the suspicion question. Again, when participants had information about their partners, they thought that their partners would be better at detecting their lies ($F(2, 549) = 4.98$, $P = 0.007$, $\eta_p^2 = 0.02$). Post hoc tests show that this holds for both the one fact condition (Bonferroni-corrected $P = 0.01$) and the four facts condition (Bonferroni-corrected $P = 0.05$), when compared to the no information condition. Responses in the two information conditions did not significantly differ (Bonferroni-corrected $P = 1.00$).

These results suggest that participants believe that known strangers will be more attuned to specific details about them. Our final laboratory experiments explored whether this shift in perception also leads to a shift in behaviour.

Honesty around known strangers

In experiment 3A ($n = 1{,}010$), participants were informed that they would be asked whether they had ever had a certain experience. If they had and they wrote a description about it, they would be eligible for a bonus payment. If they never had the experience, they were not eligible for the bonus payment. To ensure that participants responded honestly, they were told that their partner would flag any responses that seemed dishonest. If their partner flagged their response, they would not earn the bonus. Thus, there was an incentive (the bonus payment) to lie to say they had the experience even if they had not, but only if they believed that they would not be caught.

The information manipulation was as in experiment 1A. After being told that they were connected to a partner, participants first indicated how much they felt they knew their partner and how much they felt their partner knew them (eight participants were excluded for missing responses). Next, participants were asked whether they had ever had surgery (15 participants were excluded for missing responses). Participants then completed the suspicion check (six participants were excluded for missing responses).

Because participants were randomly assigned to the information conditions, the proportion of participants who said they had undergone surgery should not vary by condition. Therefore, a higher proportion of participants indicating that they had undergone surgery would suggest more dishonest reporting (to try to earn the monetary bonus). More participants said they had surgery in the no information condition (49.3% versus 41.6% in the information condition, $\chi^2 = 5.91$, $P = 0.02$, Cramer's $V = 0.08$), indicating that participants were more honest around known strangers.

Moreover, participants in the information condition felt as if they knew their partner better (information: $M = 4.55$, 95% CI = 4.39–4.72 versus no information: $M = 3.89$, 95% CI = 3.70–4.08), unequal variances t-test $t(979.85) = 5.23$, $P < 0.001$, $d = 0.33$). In addition, participants in the information condition felt as if their partner knew them better (information: $M = 4.32$, 95% CI = 4.15–4.49 versus no information: $M = 3.94$, 95% CI = 3.76–4.13), unequal variances t-test $t(994.87) = 2.96$, $P = 0.003$, $d = 0.19$). Participants' feelings of knowing their partner and their feeling of their partner knowing them significantly mediated the effect of information on (dis)honesty (knowing their partner: indirect effect = −0.09, bias-corrected 95% CI = −0.15 to −0.04; their partner knowing them: indirect effect = −0.05, bias-corrected 95% CI = −0.11 to −0.02; for serial mediation, see Supplementary Information A.3). Suspicion did not significantly differ across conditions (75.7% believed that they were connected in the information condition versus 75.9% in the no information condition; $\chi^2 = 0.01$, $P = 0.92$; using TOST[19], we can reject a difference of more than 10%, $Z = 3.62$, $P < 0.001$).

The procedure in experiment 3B ($n = 582$) was identical, but without the suspicion check. Again, participants in the information condition were more honest ($\chi^2 = 4.69$, $P = 0.03$, Cramer's $V = 0.09$), felt that they knew their partners better ($t(580) = 4.96$, $P < 0.001$, $d = 0.41$; mediation analysis: indirect effect = −0.15, bias-corrected 95% CI = −0.25 to −0.07]) and felt that their partners knew them better ($t(580) = 2.40$, $P = 0.02$, $d = 0.20$; mediation analysis: indirect effect = −0.09, bias-corrected 95% CI = −0.18 to −0.01]). Experiment 3C ($n = 299$; five participants were excluded for missing responses) only included the honesty measure, and we found the same results ($\chi^2 = 6.07$, $P = 0.01$, Cramer's $V = 0.14$).

These final laboratory experiments highlight the behavioural consequences of overlooking asymmetries in social ties. People appear to be more honest around known strangers.

Next, we explored a policy implication of these findings—namely, whether they can help to unpack important mechanisms that underlie community policing strategies, particularly in large cities where officers are less connected to the areas they serve[21]. Community policing initiatives attempt to increase this connection, but little is known specifically about the effects of residents learning more about individual neighbourhood officers. In a field experiment, we examined how providing residents with information about neighbourhood officers might affect residents' perceptions and actions.

Field experiment

We partnered with the NYPD and NYCHA to develop an intervention in which community residents received information about neighbourhood coordination officers (NCOs) who were part of a community policing programme. The intervention had two parts. First, three mailers were sent to every apartment in the treatment developments. Each mailer included an outreach card and letter about the NCO, describing mundane information about the officer, such as their favourite food, their hobbies or why they became an officer (Extended Data Figs. 1, 2). Mailers were sent at approximately 3-week intervals between November 2017 and January 2018. The second part of the intervention consisted of a day in which each NCO handed out their outreach cards to residents at a time when they would already be patrolling the development.

We first paired eligible NYCHA housing developments ($n = 69$) on the basis of community characteristics. We then randomized developments within pairs to either treatment or control (for development characteristics and balance test, see Extended Data Table 1). Control developments did not receive mailers or outreach cards. On the basis of our laboratory results, we expected that residents who received information about their NCOs would believe that neighbourhood officers would be more likely to know things about the residents (for example, whether they did something illegal), and residents might be less likely to engage in illegal activity.

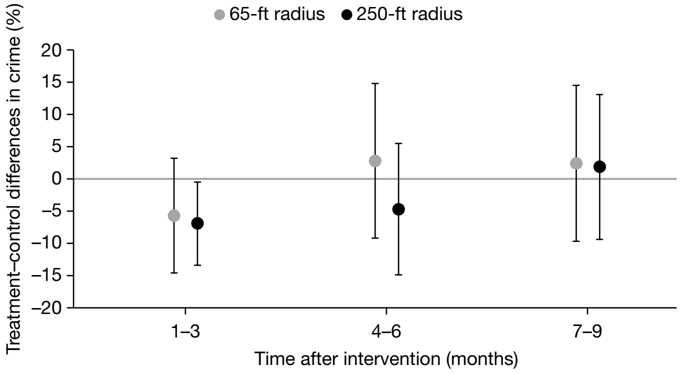

Fig. 1 | Treatment-control differences in crime after policing intervention. Providing residents with information about their neighbourhood officers reduced crime near housing developments in the first 3 months after the intervention. Changes in on-campus (grey) and near-campus (black) crimes reported post-intervention are shown, where $n = 39$ treatment developments and $n = 30$ control developments. Point estimates represent per cent change attributed to the intervention in crimes per resident per month (error bars represent 95% confidence intervals). Estimates are based on our primary specification (Supplementary Information C.3) for 1–3 months (on-campus, $P = 0.21$; near-campus, $P = 0.04$), 4–6 months (on-campus, $P = 0.68$; near-campus, $P = 0.36$), and 7–9 months (on-campus, $P = 0.70$; near-campus, $P = 0.74$) post-intervention. P values are from two-tailed tests.

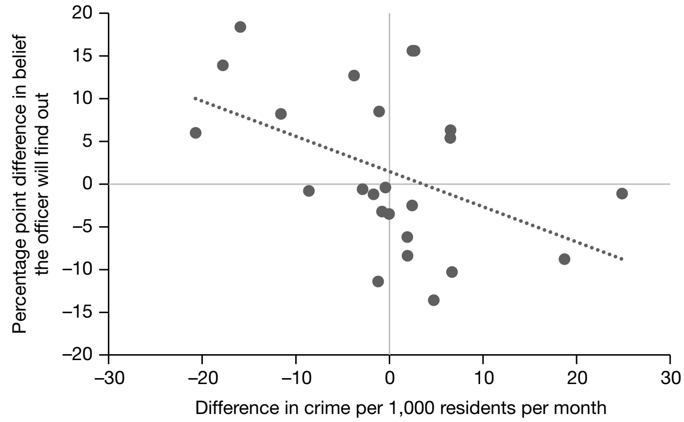

Fig. 2 | Residents' perceptions of officer knowledge predict crime reductions. For each randomization pair, we calculated treatment-control differences for crime within 250 ft of the development during the first 3 months after the intervention and residents' beliefs about whether officers would find out whether they did something illegal (for analysis details, see Supplementary Information C.6). Further left on the x axis corresponds to larger decreases in crime; further up on the y axis corresponds to larger increases in residents' beliefs about officers' knowledge. A significant correlation exists such that development pairs in which the intervention resulted in larger increases in beliefs that officers would find out whether they did something illegal also showed larger decreases in crime after the intervention ($r(24) = -0.45$, $P = 0.03$, two-tailed).

Approximately 2 months after the intervention, we conducted a survey with residents ($n = 1,858$) across treatment and control developments. To assess residents' perceptions of what officers knew about them, residents were asked (1) how likely is it that a neighbourhood officer would know whether the resident did something illegal and (2) how well neighbourhood officers knew the resident in general (on five-point scales). The intervention led to a 0.13 standard deviation increase in the average NYCHA resident's belief that an officer would find out whether they committed a crime (control $M = 3.2$, 95% CI of treatment effect = 0.03 to 0.23 s.d., $P = 0.02$). We did not find a statistically significant effect on whether residents felt that officers knew them more generally (control $M = 1.8$, 95% CI of treatment effect = −0.11 to 0.13 s.d., $P = 0.86$). Results are also robust across various alternative specifications (see Extended Data Table 2, Supplementary Information C.2 for further details). These results echo the findings from the laboratory experiments. When residents had more information about neighbourhood officers, they believed that officers would be more aware of their illegal activity. However, these effects did not generalize to their perceptions of what officers knew about them in general. The intervention also did not affect the answers of residents to questions about police responsiveness in the area, officer familiarity with the area, the trust of residents in the police and other sentiments towards officers working in their area (Extended Data Table 3).

Next, we analysed the effects of the intervention on reports of criminal activity. Our pre-registered analyses (https://osf.io/mkgwr/) focused on criminal complaints and arrests within a 65-foot radius of the development (that is, 'on-campus') and a 250-foot radius of the development (that is, 'near-campus', equal to an approximately one-block radius) over 9 full months post-intervention.

Figure 1 depicts the regression results for our primary crime analysis: the number of criminal complaints per 1,000 residents per month, analysed 1–3 months, 4–6 months, and 7–9 months after the intervention (see Methods and Supplementary Information C.3 for details on regression specification and covariate selection). For the first 3 months after the intervention, our point estimates suggest a 5.7% reduction in on-campus crimes and a 6.9% reduction in near-campus crimes. The point estimates are similar in magnitude, but our estimates are more precise for near-campus crimes, in which we see a significant reduction (control $M = 11.7$, 95% CI of treatment effect = −13.4 to −0.5%, $P = 0.04$). These effects fade out over time, which may be due to the light-touch, limited-duration nature of the intervention.

We tested the robustness of these crime results in several ways (Supplementary Information C.3). For instance, these results are robust across different intent-to-treat and treatment-on-the-treated analyses, alternative covariate sets, and estimating the effect on total crimes instead of crimes per person (Extended Data Tables 4, 5). We also conducted analyses that vary the radius around the housing developments and the time interval after the intervention. Across these specifications, the point estimates of the treatment effect are quite consistent with our main results (Extended Data Figs. 3, 4), which appear to slightly understate the duration (over time) and reach (over distance) of the effects of the intervention.

To further test the mechanism behind these crime reductions, we compared (at the level of randomization pair) treatment–control differences in the belief that officers would find out whether residents did something illegal with treatment–control differences in crime within 3 months of the intervention (Supplementary Information C.6). This analysis was not part of our pre-analysis plan but stemmed from helpful referee suggestions. Development pairs in which the intervention resulted in larger increases in beliefs that officers would find out whether residents did something illegal also showed larger decreases in crime after the intervention ($r(24) = -0.45$, $P = 0.03$) (Fig. 2). Further analyses addressed other potential mechanisms and explanations for the crime reductions. For instance, we did not find evidence that the intervention reminded residents of police presence or that it changed the behaviour of officers (Supplementary Information C.6).

We also see, through the first 3 months, a marginal increase in arrests per crime both on-campus (control $M = 0.47$, 95% CI of treatment effect = −0.7 to 23.0%, $P = 0.07$) and near-campus (control $M = 0.48$, 95% CI of treatment effect = −1.1 to 20.4%, $P = 0.08$; for more discussion see Supplementary Information C.4). Finally, given the persistent racial and ethnic disparities in policing[22–24], we tested for heterogeneity in the treatment effects based on the racial and ethnic demographics of the development, although we did not find significant heterogeneity (Supplementary Information C.7).

Discussion

These results suggest that people sometimes overlook the asymmetry that exists with known strangers. Future research will need to examine when people can recognize these asymmetries and when they overlook them. However, these findings indicate that our sense of anonymity can depend not just on what people know about us but also on what we know about them.

Putting the policing intervention in context, a recent meta-analysis found that hotspot policing tactics that increase the police presence in an area reduce crime by 0.11 s.d. on average[25]. Without increasing police presence, our intervention reduces on-campus and near-campus crime by 0.14 s.d. and 0.17 s.d., respectively, within the first 3 months post-intervention. These effects are short-lived, but fade-out is also common among other hotspot policing strategies[26,27].

These findings shed light on one aspect of community policing strategies. To the extent that these strategies allow residents to learn more about officers (even if not an intentional focus of these policies), the known stranger effect could be a mechanism behind observed crime impacts. This may be one reason why door-to-door visits from officers are more effective at reducing crime than other components of community policing such as neighbourhood watches or storefront offices[26–28]. In addition, because sharing information about officers can be easily implemented, it seems feasible to integrate this approach into many existing policing strategies.

There is certainly no panacea for important policy challenges such as policing. Broader reforms are necessary to reduce disparities and to increase trust in policing[29–31]. There are also questions about whether residents actually benefit from a focus on policing as the primary lever for crime prevention[32]. Yet, however community members and policymakers decide on acceptable levels of policing (versus other social services and policies), there may be ways to reduce crime without increasing the number of potentially fraught officer–citizen interactions. One way to do that might be to provide community members with more information about neighbourhood officers.

More generally, these results suggest an interesting wrinkle in the psychology of anonymity and social interactions. There is an increasing number of ways for people to interact with others about whom they know very little. Milgram suggested that the "ultimate adaptation to an overloaded social environment is to ... develop highly efficient perceptual means of determining whether an individual falls into the category of friend or stranger"[12]. It appears, however, that there is a profound imprecision in how we perceive these categories. We seem to assume that others see us as we see them.

Online content

Any methods, additional references, Nature Research reporting summaries, source data, extended data, supplementary information, acknowledgements, peer review information; details of author contributions and competing interests; and statements of data and code availability are available at https://doi.org/10.1038/s41586-022-04452-3.

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Methods

Sample size determination and randomization

For the laboratory experiments, sample sizes were determined before collecting data (and no additional data were collected for a study after the analysis began). We used informal rules of thumb to set the target sample sizes. For the experiments from each paradigm that were conducted first chronologically (see the Reporting Summary for study chronology), we aimed for 150–200 participants per condition (although this was not always reached), which would be considerably larger than the typical sample size from related studies on the illusion of transparency[10]. For follow-up studies, target sample sizes all had at least as many participants per cell, but power analyses were not conducted. Participants were randomly assigned to condition after submitting the online consent form.

For the field experiment, housing developments were included if the NCOs serving them agreed to participate and were eligible to do so based on their assignments. For the resident survey, we set a target of 30 responses per development.

Data analysis and reporting

Analyses were conducted in SPSS (v.27), STATA SE (v.16.1), R (v.3.6.1) and Rstudio (v.1.2.5001). We calculated effect sizes for the results from the laboratory experiments using either Cohen's *d*, partial eta-squared or Cramer's *V* (using SPSS).

Laboratory experiments

Research protocols (which included deception) were approved by the Institutional Review Board at the University of Chicago. All participants provided informed consent. We report all conditions, measures and data exclusions.

We used three experimental paradigms, with replications and extensions within each. For each, we lead with the final replication conducted because those studies included checks for suspicion about the deception. For further details about the text of questions, see Supplementary Information A.

Experiment 1A. Four hundred and two participants (M_{age} = 40.1; 190 women and 211 men; the demographics were missing for one participant) completed this experiment. Participants were first asked to complete an icebreaker, which consisted of multiple choice questions about the type of area they live in, their marital and family status, and their work status. We selected these questions because they are generic, and knowing someone's answers to them is minimally informative.

Participants then saw a screen that made it seem as if the survey was searching for and connecting them to a partner. Participants were randomly assigned to one of two conditions. In the no information condition, participants were told "Because of the role that you have been assigned, we will not be showing you any information about your partner". In the information condition, participants were told "Because of the role that you have been assigned, we will show you all of your partner's responses below". In the information condition, the partner's responses were randomly generated.

Participants were then told that their marital and family status had been shared with their partner, but that no other information was shared with their partner. On the next page, participants were told that their partner was trying to guess the participants' answers to the questions about the type of area they live in and their work status. Participants were asked to rate how much they felt their partners understood them. Participants were shown ten images of icebergs with varying degrees of ice hidden below the surface and ice visible above the surface. Responses with higher numbers corresponded to a greater feeling of being visible to and understood by one's partner (adapted from ref. [18]). As a suspicion check, participants were then asked whether they believed they were connected to another person.

The responses of five participants were missing for the question about how well they felt their partners knew them, yielding n = 397 for that measure. The response of one participant was missing for the question about whether they believed they were connected to a partner, yielding n = 401 for that measure.

Experiment 1B. Two hundred and ninety-one participants (M_{age} = 35.3; 129 women and 161 men; demographics were missing for one participant) completed the experiment. The procedure was identical to experiment 1A, but without the suspicion check.

Experiment 1C. This experiment was designed to test the possibility that the effects in experiments 1A–1B were driven by participants who felt similar to their partners, rather than merely knowledgeable about them. When participants were given information about their partners, some of that information might have matched the participants' own responses to the icebreaker questions. If so, then participants in the information condition might have felt that their partners better understood them simply because they perceived their partners to be more like them.

Four hundred and fifty-seven participants (M_{age} = 32.2; 164 women and 291 men; demographics were missing for two participants) completed this experiment. The procedure was essentially identical to experiment 1A. However, there were two information conditions in this experiment. In the 'dissimilar' condition, the partner's responses to the icebreaker questions were generated such that none would match the participant's responses. In the 'similar' condition, the partner's responses were generated such that they matched the participant's responses on two of the questions. We chose not to have the responses match on all three questions to reduce suspicion that the responses were fabricated. The response of one participant to the iceberg scale was missing, yielding n = 456 for the analysis.

Experiment 1D. This experiment was designed to test the possibility that the effects in experiments 1A–1C were due to potential confusion over what information had been shared with the participant's partner (that is, participants in the information condition might have thought that more of their icebreaker responses were shared with their partner).

Five hundred and forty-eight participants (M_{age} = 35.1; 225 women and 321 men; demographics were missing for two participants) completed this experiment. The procedure was essentially identical to experiment 1A. However, participants did not answer any demographics questions themselves. Instead, participants read the three demographics questions and were told that their partner had answered them. In addition, instead of using the iceberg measure, participants were simply asked how much they would feel like their partner knew them if they were to meet (on a seven-point scale). Responses were missing for five participants, yielding n = 543 for the analysis.

Experiment 2A. Four hundred and sixty-two participants (M_{age} = 39.9; 237 women and 225 men) completed this experiment. For the icebreaker, participants were asked to write down four true statements about themselves and one lie about themselves. Participants then saw a screen that made it seem as if the survey was searching for and connecting them to a partner. Participants were then told that they were connected to another participant, who was viewing their five statements.

Participants were randomly assigned to one of two conditions. In the no information condition, participants were not shown any statements about their partner. In the information condition, participants were shown four statements about their partner (Supplementary Information A.2).

Participants were then asked how likely (as a percentage) they thought it was that their partner would guess which of their five statements was a lie. Participants then indicated how well they felt they knew their partner (on a seven-point scale) and whether they believed they were connected to another person.

Experiment 2B. Five hundred and fifty-two participants (M_{age} = 35.2; 268 women and 283 men; demographics were missing for one participant) completed this experiment. The procedure was nearly identical to experiment 2A, but there were three conditions: no information, one fact (participants were shown only one of their partner's truths) and four facts (identical to the information condition above). We included two information conditions because we were interested in whether participants would feel less anonymous as they learned more about their partner. Participants were not asked how well they knew their partner or the suspicion question.

Experiment 3A. One thousand and ten participants (M_{age} = 37.3; 425 women and 581 men; demographics were missing for four participants) completed this experiment. Participants were told that the experiment was part of a broader effort to collect people's descriptions of various life experiences. They were told that they would be asked whether they had a particular life experience. If they said, 'yes', then they would be asked to write a description about it. Writing the description would make them eligible for a bonus payment (which would be raffled off to eligible participants). Participants were asked to only write about things they had actually experienced.

However, because participants could earn more money by falsely reporting that they had experienced something, there was an incentive to lie (that is, to write about something they had never experienced). Participants were therefore told that they would be connected to a partner who would serve as a judge. The judge would reject any responses that seemed dishonest (rendering it impossible for the participant to earn a bonus).

Participants then saw a screen that told them they were connected to a judge. Participants in the no information condition were simply told that they were connected to a judge and were shown an ID number for the judge. Participants in the information condition were also told that the judge had responded to the three demographics questions from experiments 1A–1D, and they were shown the judge's responses (which were randomly generated). All participants were then asked how much they felt as if their judge knew them and how much they felt as if they knew their judge.

Participants were then asked about the same life experience—whether they had ever undergone surgery. They were again encouraged to respond honestly. If participants indicated they had undergone surgery, then they were asked to write about the experience. Participants were then asked whether they believed they were connected to another person. All participants were told at the end of the experiment that their responses had been accepted.

Responses of six participants were missing for the question about whether they believed they were connected to a partner, yielding n = 1,004 for that measure. Responses of 15 participants were missing for the question about whether they had undergone surgery, yielding n = 995 for that measure. Responses of eight participants were missing for the questions about how well their partner knew them and how well they knew their partner, yielding n = 1,002 for those measures.

Experiment 3B. Five hundred and eighty-two participants (M_{age} = 39.8; 296 women and 284 men; demographics were missing for two participants) completed this experiment. The procedure was nearly identical to experiment 3A, but without the suspicion check.

Experiment 3C. Two hundred and ninety-nine participants (M_{age} = 35.9; 115 women and 181 men; demographics were missing for three participants) completed this experiment. The procedure was nearly identical to experiment 3A, but without the questions asking how well participants felt they knew their partner and how well they felt their partner knew them, and without the suspicion check. Responses were missing for five participants, leaving n = 294 for the analysis.

Field experiment

The field intervention focused on informing residents in NYCHA developments about NCOs assigned to their housing development. The NYCHA developments in our sample represent some of the more disadvantaged areas in New York City, constituting 1.3% of the population of the city, but accounting for 3.0% of all crimes, 3.5% of violent crimes and 5.8% of shootings reported to the police in New York City in 2016. NCOs are part of the NYPD's Neighbourhood Policing strategy meant to build connections between officers and community residents. NCOs often work the same shifts each week in defined geographical areas of a neighbourhood. They spend substantial amounts of time 'off-radio', during which they engage with residents to identify problems and work towards solutions.

Before the intervention, participating NCOs were given a survey with 22 questions that asked them about small details from their lives (ranging from their favourite food to why they became an officer; see Supplementary Information B.1 for questions). Each NCO selected three to five questions that they felt comfortable answering. Their responses were used to develop individualized outreach letters and cards that would be sent to residents to give them more information about the officers. Each outreach card included the name of an NCO, their contact information and three facts about them. Letters included similar information, but also elaborated on the three facts about the NCO and added general information about the NCO programme (sample outreach cards and letters are shown in Extended Data Figs. 1, 2).

Randomization

To evaluate the effects of the intervention, we randomly selected a subset of eligible NYCHA housing developments to receive it. NYCHA developments were considered eligible for this intervention if their associated NCOs agreed to participate. There were 38 NCOs who agreed to participate and who were also eligible to participate based on their assignments. As each NYCHA development is assigned one to two NCOs, and many NCOs are assigned to multiple developments, this resulted in 69 eligible developments. Developments were then grouped together if they were geographically close to each other and were served by the same NCO. This resulted in 55 NCO–NYCHA 'development groups'. Development groups were then paired within Police Service Areas by past crime in 2015–2016, and then randomized within pairs (see Supplementary Information B.4 for further details) to either the control group or the treatment group (which received the information intervention). Random assignment appears to have successfully balanced treatment and control groups on baseline characteristics (Supplementary Information C.1).

Intervention

The intervention had two parts. First, three mailers were sent to every apartment in the treatment developments. Each mailer included an outreach card and letter about the NCO. Mailers were sent at approximately three-week intervals from November 2017 through January 2018. The second part of the intervention consisted of a day during which each NCO handed out their outreach cards to residents at a time when they would already be patrolling the development. NCOs could choose on which day to hand out their cards. After randomization, four treatment developments did not receive the intervention because NCOs were reassigned. In addition, seven more developments assigned to treatment did not receive NCO outreach (but did receive mailers), and two developments assigned to the control group received NCO outreach (but not mailers).

Post-intervention survey

We first tested the effect of the intervention on residents' perceptions of officers via resident surveys. On the basis of our laboratory experiments, we were primarily interested in residents' perceptions of what officers knew about them. This was measured with two items. One item focused specifically on illegal activity: "Imagine that you did something in the area that an officer could write you a ticket for. How likely is it that officers in your area would know or find out about it (even if they didn't write you a ticket)? (examples: disorderly conduct, drinking alcohol in public or being on private property without permission from the owner)". This was answered on a five-point scale from 'very unlikely' to 'very likely'. The second item asked about the officer's knowledge of the resident more generally: "How well do you think the officers who work in your area know you?" This was answered on a five-point scale from 'not at all' to 'very well'. These two questions allow us to test whether the intervention only shifts residents' perceptions of officer knowledge as it relates to illegal activity, or whether the effect is more generalized. Owing to the novelty of this policing intervention, we also included a range of other exploratory measures in the survey. These included questions about police responsiveness in the area, officer familiarity with the area, the trust of residents in the police, and other sentiments towards officers working in their area (see Supplementary Information B.2 for the full survey).

We surveyed NYCHA residents (n = 1,977) across participating developments (both treatment and control) 2 months after the intervention. We did not survey residents in the four treatment developments that did not receive the intervention. Surveys were conducted by research assistants working in pairs stationed at entrances to buildings in NYCHA developments. Research assistants invited every fifth adult (18 years of age or older) entering or exiting the building to participate in the survey. Research assistants identified themselves as working with ideas42, a research partner that uses behavioural science to design solutions and inform policies. Surveys were conducted from approximately 08:00 to 18:00 between March and April 2018. Research assistants aimed for a target of 30 responses from each housing development. Residents responded to the survey on a tablet, and research assistants were available to answer any questions that residents might have. Surveys were primarily conducted in English, but Spanish and Mandarin translations were also available. Research assistants who spoke Spanish and Mandarin were also part of teams who surveyed developments with higher proportions of residents who speak those languages. If respondents expressed challenges with reading the survey, then research assistants would offer to read the survey to them.

The surveys were fairly well balanced across treatment and control developments on respondent demographic characteristics and survey response rates, although we did see significant imbalance owing largely to the fact that more Black individuals were surveyed in treatment developments, surveys in treatment developments were conducted slightly later in the day (12:35 on average as opposed to 12:15 on average), and several pollsters conducted a higher proportion of their surveys in either treatment or control developments (Supplementary Information C.2).

Survey analysis

The randomized experimental design of our field experiment meant that we could isolate the causal effects of the intervention using a simple analytic approach. Namely, we regressed the survey response for each resident on an indicator variable for whether they live in a development assigned to receive the intervention, a set of baseline demographic characteristics and crime statistics of the NYCHA development measured before the intervention, and a set of demographic characteristics for the resident at the time of the survey that include characteristics about the time of day the resident took the survey and who administered it (see Supplementary Information C.2 for further details).

Specific covariates were chosen from this full set of past crime and demographic characteristics following a double LASSO procedure[33], to select covariates in a 'hands off' and principled way, reducing researcher degrees of freedom. For each outcome, we ran the LASSO procedure 500 times and report the results with the median t-value. Results are robust to varying the parameters of the LASSO procedure. Standard errors are clustered at the development group level.

For our primary survey analysis, we dropped the control developments paired (before randomization) with the four unsurveyed developments, leading to n = 1,858 surveys. Our analyses are robust to whether we drop or include responses from control developments matched to those treatment developments, and they are robust across a variety of alternative specifications (see Supplementary Information C.2 and Extended Data Table 2 for further details).

Neighbourhood crime analysis

We then tested whether the intervention affected reports of criminal activity on and around each NYCHA development over the course of 9 months post-intervention. Our pre-registered analyses (https://osf.io/mkgwr/) focused on criminal complaints and arrests within a 65-foot radius of the development (that is, 'on-campus') and a 250-foot radius of the development (that is, 'near-campus', equal to an approximately one-block radius). We considered near-campus crimes because several developments have few crimes reported on campus, and it is feasible that any crime impacts would occur around the development, not just in the development. We initially planned to base our analyses on a restricted-use NYPD dataset. However, it became more feasible to use publicly available NYPD data, which included near-campus data and covered the full 9-month period specified in our pre-registration (see Supplementary Information B.3 for data sources).

The randomized experimental design of our field experiment meant that we could isolate the causal effects of the intervention using a simple analytic approach. Namely, we regressed each measure of development-level crime on an indicator variable for whether the NYCHA development was assigned to receive the intervention and a set of baseline demographic characteristics and crime statistics of the development measured before the intervention (see Supplementary Information C.3 for further details).

Specific covariates were chosen from this full set of development-level past crime and demographic characteristics following a double LASSO procedure[33]. For each outcome, we ran the LASSO procedure 500 times and report the results with the median t-value. Results are robust to varying the parameters of the LASSO procedure. Development-level observations are weighted by development populations. Standard errors are clustered at the development group level. Results are robust across various alternative specifications (see Supplementary Information C.3 for further details).

Reporting summary

Further information on research design is available in the Nature Research Reporting Summary linked to this paper.

Data availability

The Open Science Framework page for this project (https://osf.io/mkgwr/) includes all data from laboratory experiments and all data necessary to reproduce the results of the field experiment.

Code availability

The codes for running the laboratory experiments online and for analysing the data from the field experiment are available on the Open Science Framework (https://osf.io/mkgwr/).

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Author contributions A.K.S. developed the hypotheses. A.K.S. designed, conducted and analysed the laboratory experiments. A.K.S. and M.L. designed the field intervention. M.L. led the analysis of the field intervention. A.K.S. and M.L. contributed to the manuscript.

Competing interests The authors declare no competing interests.

Additional information

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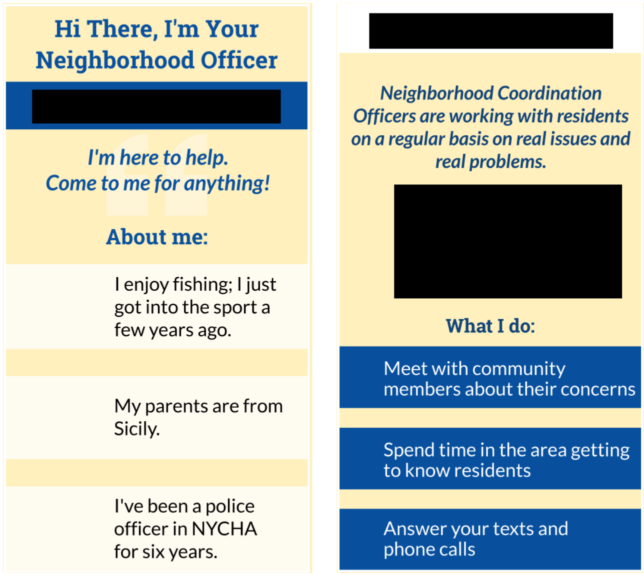


Extended Data Fig. 1 | Outreach cards. A sample outreach card (front and back) used in the field intervention. Identifying information has been redacted.

[redacted]

[redacted]

Dear Community Member,

My name is [redacted] and I wanted to introduce myself because I am one of the community officers working in your building. I am part of the Neighborhood Coordination Officer program and I do a lot more than just answer 911 calls. A lot of my time is spent meeting and working closely with community members to learn about any issues and problems in the neighborhood. I spend time every day walking around and talking with residents, business owners, teachers from the local schools, and others in the area. I am here to help with any concerns you or your neighbors may have.

I'll be spending a lot of time in the area and I hope to get to know you soon; in the meantime, here is a bit about me:

- I have been a police officer for 6 years. I spent all my time working as a police officer for NYCHA. I have worked in [redacted]
- I spend my free time fishing with my fiance. I just got into fishing a few years ago so I am still learning, but if you know any good tips I would love to hear them. Also if you want to see some pictures of my catch all you need to do is ask.
- My mother and father were born in Sicily.

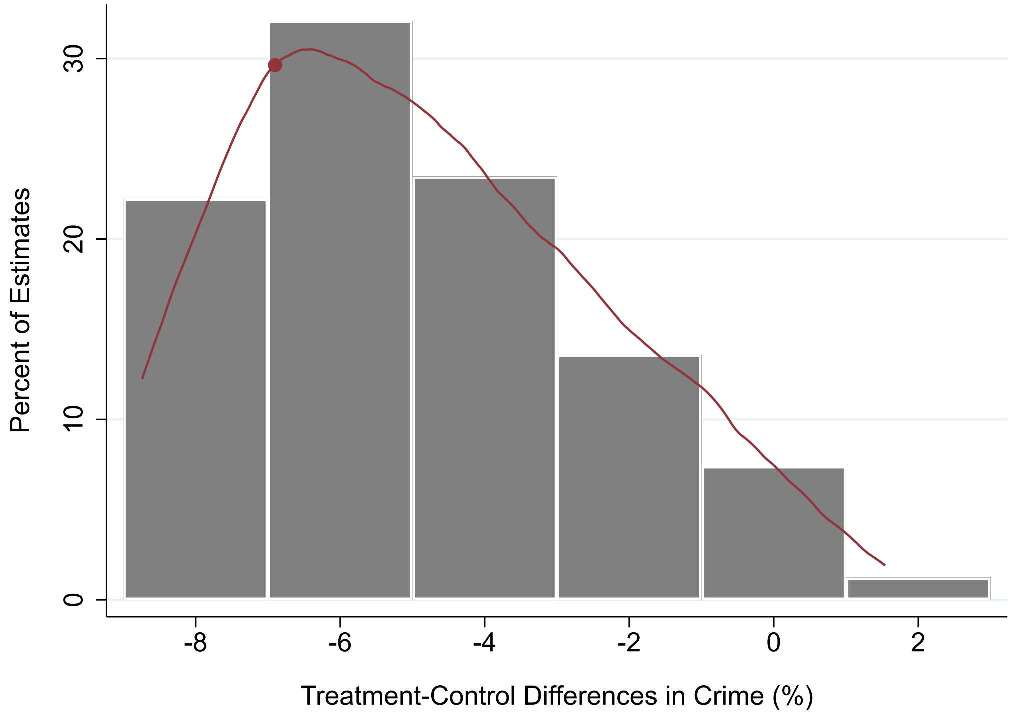
That's just a bit about me. I'm looking forward to working with the community and hope to get to know you better. **I encourage you to come to me with anything at all; I'll do my best to help.**

Looking forward to meeting you soon,

[redacted]

PS – I included my card in this letter for you to hold on to. Feel free to give me a call if you ever need anything. I will work to help out with any issue you may have in the building area.

Extended Data Fig. 2 | Outreach letters. A sample letter used in the field intervention. Identifying information has been redacted.



Extended Data Fig. 3 | Distribution of point estimates for treatment effect. As a robustness check, we conducted analyses for various radii ranging up to three blocks around developments: 65 ft., 100 ft., 150 ft., 200 ft., 250 ft., 300 ft., 400 ft., 500 ft., and 750 ft. And, for each radius, we conducted analyses for cumulative time intervals ranging from one month after the intervention (i.e., February 2018) to the first nine months after the intervention (i.e., February through October 2018). Varying both of these dimensions produced 81 sets of results, based on our primary specification applied to each radius and time interval (see Supplementary Information C.3). This figure shows the distribution of point estimates for the crime reductions across these analyses, along with an Epanechnikov kernel density function over the distribution. The red dot highlights where the 250-ft, 3-month result falls in the distribution, suggesting it is in line with the central estimates across all 81 analyses.

	1 Mo.	2 Mos.	3 Mos.	4 Mos.	5 Mos.	6 Mos.	7 Mos.	8 Mos.	9 Mos.
65ft	0.576	0.904	0.206	0.262	0.769	0.991	0.934	0.984	0.841
100ft	0.379	0.966	0.145	0.192	0.386	0.602	0.622	0.521	0.466
150ft	0.398	0.798	0.185	0.131	0.208	0.273	0.282	0.363	0.318
200ft	0.335	0.828	0.098	0.037	0.098	0.159	0.288	0.373	0.379
250ft	0.243	0.802	0.036	0.028	0.075	0.109	0.201	0.274	0.306
300ft	0.242	0.622	0.051	0.018	0.048	0.065	0.099	0.147	0.193
400ft	0.178	0.635	0.027	0.010	0.045	0.020	0.043	0.093	0.133
500ft	0.182	0.692	0.018	0.008	0.044	0.034	0.061	0.156	0.210
750ft	0.261	0.561	0.058	0.017	0.057	0.047	0.093	0.205	0.303

Extended Data Fig. 4 | Heat map of P-values for treatment effect over time and distance. As a robustness check, we conducted analyses for various radii ranging up to three blocks around developments: 65 ft., 100 ft., 150 ft., 200 ft., 250 ft., 300 ft., 400 ft., 500 ft., and 750 ft. And, for each radius, we conducted analyses for cumulative time intervals ranging from one month after the intervention (i.e., February 2018) to the first nine months after the intervention (i.e., February through October 2018). Varying both of these dimensions produced 81 sets of results. This figure shows a heat map of *P*-values across these 81 specifications, with the 250-ft, 3-month result outlined in blue. *P*-values are from two-tailed tests based on our primary specification applied to each radius and time interval (see Supplementary Information C.3).

Extended Data Table 1 | NYCHA development characteristics

	Control Mean	Treatment Mean	*P*-value
Development Level Data			
Total Population	1,795	1,569	0.53
% Black	40%	45%	0.25
% Hispanic	49%	47%	0.64
% Other Race	9%	5%	0.21
% Working Families	49%	49%	0.86
Average Income	25,209	25,624	0.64
% Public Assistance	13%	14%	0.52
Census Block Group Level Data			
% Female	56%	57%	0.48
Median Age	34	33	0.64
High School Graduate Rate	69%	71%	0.64
College Graduation Rate	26%	26%	0.97
Unemployment Rate	17%	15%	0.64
Labor Force Participation Rate	52%	53%	0.69
Crimes and 311 Calls Pre-Intervention (per 1000 residents per month)			
Crime Complaints	11.3	11.8	0.69
Arrest Rate per Reported Crime	58%	63%	0.33
Arrests	6.8	7.3	0.70
311 Calls	21.6	17.8	0.42
NYCHA Broken Property Work Orders	16.1	18.8	0.24
F-test for joint significance			0.15

Demographic characteristics and pre-intervention outcomes for control (n=30) and treatment (n=39) developments. Randomization successfully balanced these variables across treatment and control ($F(18, 50)$=1.45; P=.15 based on a two-tailed test; standard errors unclustered to provide a conservative estimate of balance). Development-level data is from 2017, with the exception of racial demographics (which are from 2016). Census block group level data is from ACS five-year rolling averages from 2012–2016. Crime and 311 call data are averaged across the three-year period prior to the intervention (November 2014 - October 2017).

Extended Data Table 2 | Primary survey outcome estimates

	Primary Specification		Alternative Covariate Set 1		Alternative Covariate Set 2		Keep Matched Control Devs		Drop Lowest Control Devs	
	Estimate	*P-value*	Estimate	*P-value*	Estimate	*P-value*	Estimate	*P-value*	Estimate	*P-value*
Officer Awareness of You										
Officer Find Out	**7.8%**	*0.02*	**5.6%**	*0.05*	**7.6%**	*0.01*	**6.7%**	*0.02*	4.8%	*0.10*
Officer Know You	1.4%	*0.86*	0.5%	*0.91*	2.4%	*0.75*	-1.8%	*0.84*	-6.1%	*0.45*

Estimates of treatment effects on residents' perceptions of how likely it was that officers would find out if they did something illegal and how well officers knew them in general. Estimates presented as the percent change in resident survey response attributed to the intervention. Our primary specification presents results when covariates are selected using the double LASSO procedure. "Alternative Covariate Set 1" includes a large set of development-level covariates (crimes per resident one, two, and three years prior to the intervention, total population, % Black, % Hispanic, % working families, and randomization pair indicators). "Alternative Covariate Set 2" includes a small set of development-level covariates (crimes per resident in the year prior to the intervention, total population, % Black, and % Hispanic). "Keep Matched Control Devs" leaves the control developments paired with the four un-surveyed developments in the dataset. "Drop Lowest Control Devs" instead drops the four control developments with the lowest average response to the survey question, among all control developments in the study. *P*-values are from two-tailed tests.

Extended Data Table 3 | Exploratory survey outcome estimates

	Control Mean	Estimate	*P-value (Q-value)*
Officer Quality			
Job quality	3.0	0.4%	*0.92 (.92)*
How much officers listen	3.1	0.6%	*0.82 (.92)*
How much officers understand issues	3.2	-1.3%	*0.65 (.92)*
Officer Responsiveness			
Patrol frequency	3.4	0.7%	*0.85 (.92)*
How many officers in area	2.7	4.4%	*0.35 (.92)*
Response speed	2.9	3.6%	*0.26 (.92)*
Cooperation			
Likelihood of reporting a crime	3.5	2.3%	*0.38 (.92)*
Likelihood of reporting suspicious activity	3.6	1.5%	*0.54 (.92)*
Officer Awareness of Others			
Awareness of illegal activity	3.6	4.3%	*0.07 (.66)*
Know what is going on in community	3.2	2.4%	*0.45 (.92)*
Knowledge of Officers			
How well do you know officers	1.8	4.5%	*0.64 (.92)*
How much in common with officers	2.0	1.6%	*0.81 (.92)*
Attitudes Towards Police			
Trust	2.8	0.9%	*0.89 (.92)*
Appreciate	3.4	1.2%	*0.68 (.92)*
Fear	2.2	12.1%	*0.03 (.41)*
Anger	1.9	7.3%	*0.43 (.92)*
Knowledge of NCO Program			
Met NCO	0.10	14.0%	*0.53 (.92)*
Heard about NCO program	0.19	6.8%	*0.66 (.92)*

Responses to survey questions vary from 1 to 5, except questions about knowledge of the NCO program, which are binary (1=yes). Estimates are presented as the percent change in resident survey response attributed to the intervention. Because the lab experiments did not provide us with a priori predictions about the intervention's effects on these measures, and given the large number of these exploratory outcomes, we correct the *P*-values for multiple hypothesis testing. Unadjusted *P*-values (from two-tailed tests) are presented in the last column along with Family-wise Error Rate adjusted *Q*-values (in parenthesis) adjusted for n=18 outcomes.

Extended Data Table 4 | Crime outcome estimates

	Primary Specification		Alternative Covariate Set		Include NYPD Precinct Crimes		Total Crime Counts	
	Estimate	*P-value*	Estimate	*P-value*	Estimate	*P-value*	Estimate	*P-value*
1-3 Months Post Intervention								
65 ft. Radius	-5.7%	*0.21*	-8.1%	*0.20*	-5.6%	*0.21*	-6.4%	*0.13*
250 ft. Radius	**-6.9%**	*0.04*	**-9.1%**	*0.04*	-5.0%	*0.11*	**-7.2%**	*0.04*
4-6 Months Post-Intervention								
65 ft. Radius	2.8%	*0.65*	5.5%	*0.42*	0.9%	*0.88*	3.2%	*0.58*
250 ft. Radius	-4.7%	*0.36*	-2.9%	*0.65*	-3.3%	*0.51*	-4.8%	*0.31*
7-9 Months Post-Intervention								
65 ft. Radius	2.4%	*0.70*	6.2%	*0.42*	1.5%	*0.79*	7.3%	*0.18*
250 ft. Radius	1.9%	*0.74*	-3.3%	*0.65*	2.0%	*0.71*	5.1%	*0.26*

The first three specifications show the percent change in crimes per resident, the final specification shows the percent change in total crimes. Our primary specification presents results when covariates are selected using the double LASSO procedure. The "Alternative Covariate Set" specification includes covariates for the most relevant development-level past-crime data over the three years prior to the intervention (crimes per resident one, two, and three years prior to the intervention), aggregate development-level population characteristics (total population, % Black, % Hispanic, % working families), and randomization pair indicators. The "Include NYPD Precinct Crimes" specification includes crimes reported at police precincts. *P*-values are from two-tailed tests.

Extended Data Table 5 | Crime outcome treatment-on-the-treated estimates

	Received Mailer		Received Outreach		Received Mailer AND Outreach		Received Mailer OR Outreach	
	Estimate	*P-value*	Estimate	*P-value*	Estimate	*P-value*	Estimate	*P-value*
1-3 Months Post Intervention								
65 ft. Radius	-6.2%	*0.19*	-7.3%	*0.19*	-6.5%	*0.19*	-6.8%	*0.19*
250 ft. Radius	**-7.3%**	*0.03*	**-8.2%**	*0.03*	**-7.6%**	*0.03*	**-7.9%**	*0.03*
4-6 Months Post-Intervention								
65 ft. Radius	3.0%	*0.64*	3.6%	*0.63*	3.2%	*0.64*	3.3%	*0.64*
250 ft. Radius	-4.9%	*0.35*	-5.6%	*0.36*	-5.1%	*0.35*	-5.4%	*0.35*
7-9 Months Post-Intervention								
65 ft. Radius	3.0%	*0.62*	3.5%	*0.62*	3.2%	*0.62*	3.3%	*0.62*
250 ft. Radius	1.9%	*0.73*	2.1%	*0.73*	2.0%	*0.73*	2.0%	*0.73*

Specifications show the percent change in crimes per resident attributed to the intervention for four TOT analyses. For the "Received Mailer" analysis, treated corresponds to receiving NCO mailers (four randomly assigned treatment developments did not receive mailers). For the "Received Outreach" analysis, treated corresponds to receiving NCO outreach (11 randomly assigned treatment developments did not receive NCO outreach, while two randomly assigned control developments did receive NCO outreach). For the "Received Mailer AND Outreach" analysis, treated corresponds to receiving NCO mailers and NCO outreach. For the "Received Mailer OR Outreach" analysis, treated corresponds to receiving either NCO mailers, NCO outreach, or both. TOT analyses use two-stage least squares. *P*-values are from two-tailed tests.

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Statistics

For all statistical analyses, confirm that the following items are present in the figure legend, table legend, main text, or Methods section.

n/a	Confirmed	
☐	☒	The exact sample size (n) for each experimental group/condition, given as a discrete number and unit of measurement
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☐	☒	The statistical test(s) used AND whether they are one- or two-sided *Only common tests should be described solely by name; describe more complex techniques in the Methods section.*
☐	☒	A description of all covariates tested
☐	☒	A description of any assumptions or corrections, such as tests of normality and adjustment for multiple comparisons
☐	☒	A full description of the statistical parameters including central tendency (e.g. means) or other basic estimates (e.g. regression coefficient) AND variation (e.g. standard deviation) or associated estimates of uncertainty (e.g. confidence intervals)
☐	☒	For null hypothesis testing, the test statistic (e.g. F, t, r) with confidence intervals, effect sizes, degrees of freedom and P value noted *Give P values as exact values whenever suitable.*
☒	☐	For Bayesian analysis, information on the choice of priors and Markov chain Monte Carlo settings
☒	☐	For hierarchical and complex designs, identification of the appropriate level for tests and full reporting of outcomes
☐	☒	Estimates of effect sizes (e.g. Cohen's d, Pearson's r), indicating how they were calculated

Our web collection on statistics for biologists contains articles on many of the points above.

Software and code

Policy information about availability of computer code

Data collection	NYCHA resident surveys were collected using Qualtrics (March-April 2018). Lab experiment surveys were programmed in HTML, JavaScript, and PHP (last run on a server with PHP 7.3), available on the Open Science Framework (https://osf.io/mkgwr/).
Data analysis	SPSS version 27, STATA SE v16.1, R v3.6.1, and Rstudio v1.2.5001

For manuscripts utilizing custom algorithms or software that are central to the research but not yet described in published literature, software must be made available to editors and reviewers. We strongly encourage code deposition in a community repository (e.g. GitHub). See the Nature Portfolio guidelines for submitting code & software for further information.

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Policy information about availability of data

All manuscripts must include a data availability statement. This statement should provide the following information, where applicable:

- Accession codes, unique identifiers, or web links for publicly available datasets
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- For clinical datasets or third party data, please ensure that the statement adheres to our policy

The Open Science Framework page for this project contains all lab experiment data, all data necessary to reproduce the field experiment results, and our pre-registration details for the field experiment. It also includes the code necessary for running the lab experiments online and for analyzing the field experiment results.

For the field experiment, posted data are drawn from three main sources: NYC Open Data (crime complaints, arrests, and 311 call complaints), NYCHA-provided data (broken property work order data and data on aggregate resident demographic characteristics, such as population, race, % working families, average income, and % families receiving public assistance), and the American Communities Survey (resident demographic characteristics such as gender, median age, educational attainment, unemployment and labor force participation). The data posted on the OSF project page contain all of the measures relevant for our analyses.

Field-specific reporting

Please select the one below that is the best fit for your research. If you are not sure, read the appropriate sections before making your selection.

☐ Life sciences ☒ Behavioural & social sciences ☐ Ecological, evolutionary & environmental sciences

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Behavioural & social sciences study design

All studies must disclose on these points even when the disclosure is negative.

Study description

There are two types of studies: (1) Laboratory experiments with quantitative data (self-report and behavioral outcomes) and (2) A field experiment with quantitative data (self-report surveys and administrative data).

Research sample

For the lab experiments, participants were recruited from Amazon Mechanical Turk. This is not a nationally representative sample, but is a sample widely used in behavioral science research. Demographics for lab experiment samples shown below:

Experiment 1A (total N=402): Mean age = 40.1; 190 females, 211 males; demographics were missing for one participant.
Experiment 1B (total N=291): Mean age = 35.3; 129 females, 161 males; demographics were missing for one participant.
Experiment 1C (total N=457): Mean age = 32.2; 164 females, 291 males; demographics were missing for two participants.
Experiment 1D (total N=548): Mean age = 35.1; 225 females, 321 males; demographics were missing for two participants.
Experiment 2A (total N=462): Mean age = 39.9; 237 females, 225 males.
Experiment 2B (total N=552): Mean age = 35.2; 268 females, 283 males; demographics were missing for one participant.
Experiment 3A (total N=1010): Mean age = 37.3; 425 females, 581 males; demographics were missing for four participants
Experiment 3B (total N=582): Mean age = 39.8; 296 females, 284 males; demographics were missing for two participants.
Experiment 3C (total N=299): Mean age = 35.9; 115 females, 181 males; demographics were missing for three participants.

For the field experiment survey, current NYCHA residents in treatment developments (N = 39) and control developments (N = 30) were surveyed, as they were directly affected by the intervention. This was a convenience sample of NYCHA residents (N = 1977) that is not necessarily representative of all residents (demographics described in Table A2).

Sampling strategy

Participants recruited from Amazon Mechanical Turk could enroll in the study by accepting a task posted to the platform. Sample sizes for each study were determined before collecting data (and no additional data were collected for a study after the analysis began). Formal power analyses were not conducted. We used informal rules of thumb to set the target sample sizes for the experiments from each paradigm that were conducted first chronologically (Experiments 1B, 2B, 3C; see note under "Data Collection"), and follow-up sample sizes all had at least as many participants per cell. Our final sample sizes are generally at least 2.5x larger than sample sizes used in similar work (e.g., experiments on the illusion of transparency).

For the field experiment, housing developments were included if the NCOs serving them agreed to participate and were eligible to do so based on their assignments.

For the resident survey, sample size was based on funding and research assistant capacity. We set a target of 30 responses per development. Surveys were conducted by research assistants (RAs) working in pairs stationed at entrances to buildings in NYCHA developments. RAs invited every fifth adult (18 years or older) entering or exiting the building to participate in the survey. RAs identified themselves as working for ideas42.

Data collection

For the lab experiments, data were collected via online surveys.
In our final manuscript, the order in which studies are labeled and presented differs from the chronological order in which they were conducted. This was done to streamline presentation of the results. The original chronology is shown below (with current labels):
First experimental paradigm: Exp. 1B, then Exp. 1C, then Exp. 1D, then Exp. 1A.
Second experimental paradigm: Exp. 2B, then Exp. 2A
Third experimental paradigm: Exp. 3C, then Exp. 3B, then Exp. 3A.

For the resident survey, data were collected via surveys on tablets. Surveys were primarily conducted in English, but Spanish and Mandarin translations were also available. RAs who spoke Spanish and Mandarin were part of teams who surveyed developments with higher proportions of residents who speak these languages. If respondents expressed challenges with reading the survey then RAs would offer to read the survey to them (for more details see SI Section B.2). RAs were blind to the hypotheses.

Timing

The lab experiments were conducted between 2015 and 2021. For the resident survey, surveys were conducted from approximately 8:00AM to 6:00PM between March and April 2018.

Data exclusions

Data exclusions (primarily due to missing responses) for the lab experiments are noted below:

Experiment 1A: One participant's response was missing for the question about whether they believed they were connected to a partner, yielding n = 401 for that measure. Five participants' responses were missing for the question about how well they felt their partners knew them, yielding n = 397 for that measure.

Experiment 1B: No exclusions.
Experiment 1C: One participant's response to the iceberg scale was missing, yielding n = 456 for the analysis.
Experiment 1D: Responses were missing for five participants, yielding n = 543 for the analysis.
Experiment 2A: No exclusions.
Experiment 2B: No exclusions.
Experiment 3A: Six participants' responses were missing for the question about whether they believed they were connected to a partner, yielding n = 1004 for that measure. Fifteen participants' responses were missing for the question about whether they had had surgery, yielding n = 995 for that measure. Eight participants' responses were missing for the questions about how well their partner knew them and how well they knew their partner, yielding n = 1002 for that measure.
Experiment 3B: No exclusions.
Experiment 3C: Responses were missing for five participants, leaving n = 294 for the analysis.

For the resident survey, we present analyses that both include and exclude respondents from control developments matched to treatment developments that did not receive treatment.

Non-participation

We do not have a precise estimate of non-participation for the lab experiments. For the resident survey, response rates are noted in Table A2.

Randomization

Both lab and field studies randomized participants.

Reporting for specific materials, systems and methods

We require information from authors about some types of materials, experimental systems and methods used in many studies. Here, indicate whether each material, system or method listed is relevant to your study. If you are not sure if a list item applies to your research, read the appropriate section before selecting a response.

Materials & experimental systems

n/a	Involved in the study
☒	☐ Antibodies
☒	☐ Eukaryotic cell lines
☒	☐ Palaeontology and archaeology
☒	☐ Animals and other organisms
☐	☒ Human research participants
☒	☐ Clinical data
☒	☐ Dual use research of concern

Methods

n/a	Involved in the study
☒	☐ ChIP-seq
☒	☐ Flow cytometry
☒	☐ MRI-based neuroimaging

Human research participants

Policy information about studies involving human research participants

Population characteristics

See above for demographic information.

Recruitment

Participants recruited from Amazon Mechanical Turk could enroll in the study by accepting a task posted to the platform. Each "task" (or experiment) was available for 24 hours.

NYCHA resident surveys were conducted by research assistants (RAs) working in pairs stationed at entrances to buildings in NYCHA developments. RAs invited every fifth adult (18 years or older) entering or exiting the building to participate in the survey. RAs identified themselves as working for ideas42.

Because all participants provided informed consent, they may have chosen to participate based on interest in the topic. However, random assignment to condition should mitigate the role of self-selection in our primary outcomes.

Ethics oversight

All studies were approved by the University of Chicago IRB, and all participants provided informed consent.

Note that full information on the approval of the study protocol must also be provided in the manuscript.